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THE PSYCHOLOGICAL REVIEW

PSYCHOLOGY AND THE NATURAL-SCIENCE POINT OF VIEW

BY MORRIS A. COPELAND

University of Michigan

Is psychology a natural science? Can the psychologist deal adequately and satisfactorily with his subject by natural-science methods and on natural-science assumptions alone? These and the correlative questions for the social sciences are the fundamental issues in the anthropological studies today. They underlie the controversy between those who hold 'purpose' to be a fundamental category of psychology and the stimulus-response psychologists, the controversy between the Gestalt and emergent evolution theorists and the more analytical psychologists, and the controversy between introspectionist and behaviorist.

It is the purpose of this paper to point out wherein certain current psychological theories conflict with a natural-science approach to a study of the functioning of human beings and to sketch briefly a type of theory which adheres rigorously to natural-science methods and assumptions. It will be convenient to have a designation for this type of theory. Although both 'behaviorism' and 'institutionalism' have been used in many senses, they have quite frequently been used to designate the natural-science type of theory in psychology and in social science respectively, and I shall arbitrarily adopt that usage here.

Behaviorism, so conceived, is neither the use of a particular methodological device nor a partial and incomplete psychology—a view that omits the consideration of important parts of the subject. And it may be added that the 'thinking

behaviorist' is no paradox. Behaviorism shares with other psychological viewpoints the study of those phases of human activity which on the naïve level we call thought and emotion, will, desire, choice and purpose, awareness and attention, perception, memory, and imagination, meaning and evaluation. It is interested in them both ontogenetically and phylogenetically and in both their normal and their abnormal development. And it proposes to study them by all the methods appropriate to scientific inquiry.

The behaviorist does not claim to offer a final solution for these difficult problems. Rather he offers a formulation which he believes is both promising and consistent with scientific hypotheses in other fields. Behaviorism—let us be frank—is a particular Weltanschauung, or rather the working out of that Weltanschauung in the field of the study of individual behavior; its counterpart in the field of group behavior, as already noted, I shall refer to as institutionalism. The behaviorist and the institutionalist are particularly concerned that their formulations shall conform to two important canons; (1) That they shall be consistent with hypotheses in other fields, especially with the natural-evolutionary hypotheses in geology and general biology; (2) That they shall leave the door open to the solution of all psychological problems by methods of scientific observation and scientific reasoning. Behaviorism is not primarily a scientific hypothesis capable of empirical test; it is primarily a faith that scientific hypotheses capable of empirical validation can be made in the field of psychology and can be made consistent with scientific hypotheses in other fields.

It will doubtless be urged that many schools of psychology share with behaviorism this scientific faith. It may be in order, therefore, to attempt first a brief formulation of a natural-science creed. I shall put this creed, so far as it directly concerns us here, into four articles:

I. Observable objects and events are approximately uniform and predictable, if appropriately analysed and classified.

II. A complex whole is scientifically understandable (*i.e.*, predictable and controllable) from a knowledge of its parts and their structural and functional relations.

III. In the case of the simpler objects and events with which physical science deals, prediction may be either forwards or backwards in time, but control is always from antecedent to consequent—never conversely.

IV. Scientific observations and descriptive generalizations should be sharply distinguished from appraisals and normative judgments.

On the first of these articles hang all the 'laws' of science. A scientific descriptive generalization ('law' or hypothesis) is possible because of the approximate uniformity of nature. It is unnecessary to assume precise uniformity. It is sufficient for purposes of prediction and control that scientific generalizations should be of a statistical character, approximate fits to the observations. It is a prime object of science to formulate generalizations useful for prediction and control. In any scientific inquiry it is to be assumed that there are more such generalizations possible than have yet been formulated. New analyses and classifications (*i.e.* new definitions of terms) may yield new generalizations.

On the first word of this article, 'observable,' hangs also the issue between the behaviorist and certain, at least, of the introspectionists. The issue is,—What are observable objects and events? For the present it may suffice to note that in the non-anthropological sciences the observer of an object or event is never uniquely specified by the object or the event. Any particular object, condition, or event may be observed by any one of a number of persons. Upon this possibility of substituting one observer for another rests what may be called the objectivity of scientific observation.

According to the second article of our creed science proceeds in large measure by analysis.[1] A given whole—object or event—is presumed to be capable of a variety of analyses. The scientific value of an analysis consists in its fruitfulness in yielding generalizations useful for prediction and control. If generalizations are made about a class of objects or events upon the basis of an analysis, and if generalizations are made

[1] For a fuller discussion of this Article and its bearing on the Gestalt theory see An instrumental view of the part-whole relation, *J. Phil.*, 1927, 24, pp. 96–104.

about the same class directly and without analysis, the two sets of generalizations should yield observable agreement (*i.e.* be consistent) at least within the limits of accuracy of the generalizations.

The word 'control' in Articles II and III calls for a word of comment. The use of scientific generalizations for purposes of control, is primarily a technologist's task. But the scientist is interested in control [2] for the purpose of testing his generalizations, and the concept of control is important in defining whether one event is antecedent to or subsequent to another. Even where actual control is not feasible, control may be employed hypothetically. Thus an astro-physicist may inquire what changes in a system the near-approach of a star would involve, *subsequent* to that near-approach. He cannot control his condition, the near-approach of the star at a particular date, but he assumes that, if he could, control would be from antecedent to consequent, and not conversely. So also the social scientist employs control hypothetically, when he inquires how a given social change would affect *subsequent* events, other conditions remaining the same.

Before commenting further on the four articles of our creed it will be convenient to consider some more specific aspects of science. Physical science deals with certain uniformities which it assumes to hold for all time. The properties of chemical elements, of electrons, and protons are assumed to be the same today as in the planetesimal period and long before. Physical science also deals with the origin and history of the earth as a changing complex—a set of events which it assumes to be analyzable into its uniformities which are independent of time. Geology and paleontology tell us the approximate dates of the appearance of life and of man upon the earth. Biology finds that living organisms, some directly, and some indirectly through other organisms, convert inorganic objects into parts of themselves, as they grow and multiply their kind. In view of all this the behaviorist believes that the safest assumptions for him to proceed upon are (*a*) that a man is a special case of a living organism, and (*b*) that a

[2] *Cf.* the frequent use of a 'control' in biology.

living organism is a special case of physical object—a special type of complex of electrons and protons, or if you prefer, of chemical elements.

The first of these more specific assumptions may fairly be said to be a biological commonplace. The second, if less generally accepted by biologists, is certainly a working hypothesis for many of them. Some biologists and philosophers, however, while paying lip-service to both of these propositions, have sought to avoid their implications by adopting a doctrine of 'emergent evolution.' A common formulation of this doctrine has it that some wholes are not mere aggregates of their constituent parts, in the sense that a new property emerges from the combination—an emergent—which could not have been predicted from a knowledge of the parts. Thus aqueousness emerges in the combination H_2O,—it could not have been predicted from a knowledge of hydrogen or oxygen. And, similarly, life and consciousness are emergents. The corollary of this doctrine is that the new property, or emergent, invalidates any attempt to apply generalizations derived from a study of the parts in predicting the behavior of the whole.

A man may be a special case of living organism, but generalizations about the human nervous system, muscles, glands and receptor organs cannot, according to this theory, be used to predict the behavior of the total organism, because consciousness is an emergent which influences behavior.[3] And similarly, it is held that the behavior of a living organism can never be predicted from a knowledge of its inorganic constituents alone, because of the emergent, life.

To my notion such a doctrine is a mere rationalization of compartmental-mindedness. And the rationalization rests on a confusion of two quite different statements: (1) "Some present generalizations about living organisms are not deducible (predictable) from the present generalizations of physics and chemistry," and (2) "Some generalizations about living organisms can never be deduced from any generalizations of physics and chemistry that will ever be made."

[3] *Cf.* H. S. Jennings, Diverse doctrines of evolution, *Science*, 1927, 65, p. 25.

This is the perennial resort of the mystic—the leap from "science has not yet" to "science can never." The behaviorist believes that 'can' is a more promising working hypothesis than 'can't,' and 'can' is specifically asserted in the second article of the behaviorist creed, "A complex whole is scientifically understandable (*i.e.*, predictable and controllable) from a knowledge of its parts." Let us apply this article to our propositions (*a*) and (*b*).

If (*a*) a man is a special case of a living metazoan organism, a special complex of special types of cells, human activity is understandable (predictable and controllable) from a knowledge of the behavior properties of the structural parts of the human organism—muscles, glands, nerves, receptors, etc.,—not necessarily present knowledge of course, but knowledge hoped for, at any rate. Willing, thinking, feeling, etc., are ways of behaving comparable to walking, eating, and the begetting and bearing of children. Mind is a behavior trait of the observable human organism, as life is a behavior trait of animals and plants in general, and if (*b*) living organisms are a subclass of the class, complexes of chemical elements, then mind is a behavior trait of the human body in the same sense as magnetism is a behavior trait of a magnet. And mind, as a behavior trait of the human body, is presumably understandable by analyzing it into the behavior-traits of the structural parts of the body.

It is, of course, a corollary of this view that human psychology and social science are branches of anthropological physiology, and further, that biology is a branch of physical science.

At this point certain introspectionists will doubtless bring the charge of 'incompleteness' against the behaviorist. The charge is sometimes put in ontological terms, "mental states and processes are omitted." In this form the charge implies that *in addition to* the condition and activity of the human body there are certain human mental states and activities which require scientific consideration. If this means psychophysical parallelism, it is to be noted that the mental states are necessarily *ex hypothesi* mere parallels of bodily condition

and activity, and so, for purposes of prediction and control, mere duplicates—they can add nothing to the possibilities of prediction and control which rest on observation of bodily condition and activity. And Occam's razor suggests they are for scientific purposes just so much excess baggage. If, on the other hand, a non-parallelist relation is assumed between mental and bodily states and processes, so that both are necessary to prediction of behavior, then unless infants, dogs, and many abnormals and primitive peoples fall in a sharply different class from normal, civilized adults, the charge of incompleteness necessarily denies for them the approximate uniformity of observable objects and events assumed in Article I.

Another possible ontological variant of the charge of incompleteness is to hold that all observations are observations of mental states, and that behaviorism omits some, rather than all, mental states. To this form of the charge, at least a partial answer is afforded by considering the methodological question, "Does the behaviorist omit some relevant observations?" By way of a start at answering this question we may consider the case of *P*, a psychologist, who has designed an experiment in which *S*, his subject, participates. In the course of the experiment *P* asks *S* certain questions. *S* answers these questions and possibly makes other comments. As a behaviorist, *P* would observe and record the words of *S* as faithfully as if he, *P*, had been an introspectionist. *P* would also record other phases of *S*'s behavior, but the charge of incompleteness of observation presumably lodges against the omission of *S*'s verbal activity. *P* does not omit the observation of this, but it should be noted that the behaviorist does not ordinarily class what *S* says as 'scientific observation.' Nor would *P* be likely to design an experiment in which *S* would have to be trained in any particular psychological lingo, if *P* were studying general normal adult psychology. Such a qualification would presumably make his sample of *S*'s biassed, unless indeed he were studying the psychology which is peculiar to the kind of man we call a 'psychologist.'

This view raises further questions. May not *P* be his own subject? Are there not some phases of a man's behavior and condition which he alone can observe? Practically, as a matter of convenience and of the limitations of our present technique of observation, the behaviorist's answer to both of these questions is 'yes.' But the behaviorist would prefer to assume that there is no phase of a man's condition or activity for which it is necessarily impossible to devise a technique which will make it open to the observation of others. And in view of the fact that in the non-anthropological sciences there is always the possibility of substitution of one observer for another, he feels that observations in psychology and in other branches of physiology, for which such a possibility of substitution is not a present practicability, are not upon a par with other observations in respect to objectivity. He feels less certain of their reliability than he does of those where substitution is possible. The behaviorist, then, to sum up, omits nothing which the introspectionist would class as observation, but he does not evaluate the validity of these items in the same way as would some introspectionists.

It has been said that "mind as a behavior trait of body can presumably be analyzed into the behavior traits of the structural parts of the body." This requires the behaviorist to make his formulations about mind consistent with the possibility of such an analysis. But it does not confine him exclusively to this type of analysis. Other analyses are possible, and in the present state of our knowledge perhaps more fruitful. To say that human activity consists of thinking, perceiving, remembering, willing, feeling, etc., is precisely to make another sort of analysis. And similarly, in studying group behavior, the institutionalist may find it more fruitful to analyze social organization into property, language, leadership, and other institutions than into individual behavior, although he aims to keep his hypotheses consistent with the possibility of the latter type of analysis.

Thus far we have considered what may be called the behaviorist question proper, the issue of behaviorism versus introspectionism. The attempt to make psychology con-

sistent with Propositions (*a*) and (*b*) and with Articles III and IV raises another important issue, which has sometimes been called 'teleology versus mechanism.' Article III holds that the physical sciences in dealing with chemical elements and their constituents and simpler combinations find that control is always from antecedent to consequent, never conversely. But there is no blinking the fact that in animal behavior reward and punishment influence learning. And there are numerous other phases of human activity in which the end-result appears to control the selection of means. But if (*a*) a man is a special case of living organism and (*b*) a living organism is a special case of physical object, a special type of complex of chemical elements, then it becomes a problem for the behaviorist (and for those introspectionists who take a similar view of this second issue), so to formulate their views of desire, will, choice, purpose, etc., as to be consistent with the possibility of analyzing these types of human behavior into constituent sequences in which control is from antecedent to consequent. The problem, as I shall presently formulate it, becomes one of understanding purpose and desire in terms of stimulus, response and inhibition. From a natural evolutionary point of view teleological terms like purpose are not properly fundamental terms in a scientific description. The tendency to use them as such is a survival of primitive anthropomorphism and anthropocentrism. The general problem of understanding in non-anthropomorphic terms, sequences which have hitherto been interpreted teleologically, is a pervasive one in biology. Darwin's theory of natural selection aimed to deal with it as it occurs in the study of (chiefly anatomical) phylogeny. And embryology will have much to do before it has handled the anatomical ontogenetic problem in terms of a non-teleological character.

It should be added that the behaviorist does not rule out teleological interpretations altogether. He rules them out, except as temporary expedients, from scientific description. But the biologist is not satisfied with a mere description which can be used for prediction and control. (And psychology and the social sciences are, according to a natural-evolutionary

view, biological sciences.) The physiologist, for example, specifies the ends or functions which the various organs of the body serve; and this is part of his job as a scientist. 'Function' in this sense is to be distinguished from purpose. The function of an organ is a result which the observing scientist expects the organ 'normally' to bring about and in terms of which he *appraises* its behavior. It is not an end which guides or influences that behavior. Nor is the result expected of an organ directly dependent on what actually occurs either in the individual case or typically. As a teleological interpretation it does not purport to 'explain' or even describe the behavior of the organ, but rather it tells what the result *ought* to be. It still remains necessary to make a scientific description of the behavior of the organ, a description both of the normal functioning and of the functioning of the organ which fails to realize the result expected. Such a 'description' is wrong if it leads to wrong predictions; but it is no less truly the function of the stomach to digest food, when the stomach is 'out of order.' The teleological interpretation supplements but can never be a substitute for objective description. Appraisal of the behavior of an organ as appropriate to the performance of the organic function is not part of the description that makes possible prediction and control of behavior. This is the point of Article IV.—Scientific observations and descriptive generalizations should be sharply distinguished from appraisals and normative judgments.

II

So much for the general viewpoint of behaviorism. I shall now endeavor briefly to consider the terms employed in a typical behavioristic analysis and also a characteristic institutionalist term, and to employ these terms in a few illustrative problems.[4] It should perhaps be emphasized that a behavioristic account of such an activity as human thinking or choice, if purely psychological, is necessarily incomplete in a sense somewhat different from that discussed above. It

[4] The brevity of the treatment necessarily involves a good deal of over-simplification.

deals primarily with the ontogenetic aspects of the problem, and requires to be complemented by a sociological and institutionalistic consideration of human functional phylogeny and group behavior, for common-sense terms like thinking have reference to more than the behavior of any single individual taken by himself, however typical he may be.

We have said that the behaviorist regards thinking as comparable to walking, etc. As a biologist he studies these two forms of activity in the same general way,—a way in which also the physicist studies the magnetism of the magnet. (1) He defines his problems in observational terms, preferably in terms which permit the substitution of one observer for another. The observations include the condition and activity of *S*, his subject, and of *S*'s environment in successive intervals of time. If *S* is learning a maze, he thinks it pertinent to observe the maze as well as *S*. To omit the environment in studying *S*'s intellectual activity is certainly to run a chance of an incomplete account. (2) He may infer the occurrence of events and conditions which he does not observe directly. So does the physicist. The validity of such inferences, of course, is determined by an observational test: Do they lead to the *prediction* of activity or conditions of *S* which he can observe? (3) He endeavors to analyze human activity into temporal sequences in which he can control the consequent by the antecedent.

To define his problem in observational terms, then, is either to define it as the study of a type of behavior which he can identify directly by observation, or as a type which he infers to take place between a given observable antecedent situation and a predictable and observable resultant activity —the inferred intermediate being conceived as a consequent of the observed antecedent and as an antecedent of the observed consequent. Thus thinking may be defined roughly for observational purposes as the sort of activity which takes place between the oral or written confronting of *S* with a problem new to *S* and *S*'s oral or written answer. This does not, of course, preclude the possibility of inferring similar activity on *S*'s part when the problem presentation or the answer is not directly observable.

In endeavoring to analyze human activity into temporal sequences in which control is from antecedent to consequent, the behaviorist analyzes the behavior of *S* into parts, which he calls responses. A response is a part of human behavior which the behaviorist hopes to correlate with some antecedent condition or activity either in the organism or its environment, called a stimulus, so that he may, given the stimulus and other appropriate conditions, predict the response or, through the stimulus, control its occurrence, or given the response 'predict' or specify the stimulus. Because he believes that the behavior traits of *S* can some day be understood by analyzing them into the behavior traits of the muscles, glands, etc., of which *S* consists, the behaviorist defines the response as a specific pattern and sequence of receptors, nerves, muscles and glands in operation. And when he has correlated a stimulus and response, the behavior trait he attributes to *S* is called a response-pattern. On the negative side it is to be said that a 'response-pattern' is not to be defined in terms of results, as 'stirring one's coffee.' This procedure leads to teleological interpretations rather than scientific descriptive generalizations. The response-pattern is stirring one's coffee with the spoon in the right hand and with a very special type of position and movements of that hand and arm.

Nor is the stimulus-response relation a single-valued one in the mathematical sense. More than one stimulus may call out the same response. And while an appropriate stimulus is a necessary condition to the response, according to the behaviorist's hypothesis, it is not a sufficient condition.

The key to this complication may be sought in inhibition; *e.g.*, if two response patterns are so related that they cannot function simultaneously, and if these two are simultaneously stimulated, one will function and the other will be inhibited. Thus, the external stimulative situation or the condition of the organism may be such that any given response-pattern will not go off when stimulated. Indeed this is typically the case for most of the response-patterns the adult human being possesses. The typical human being has a vastly greater

number of response-patterns for which appropriate stimuli are present at any one time than could in the nature of the case function simultaneously, since many of the responses to these stimuli are mutually exclusive (*e.g.*, standing and sitting). We may speak of the group of response-patterns which are on the threshold of readiness to function at any one time, *i.e.*, not inhibited, as a dominant complex of response-patterns, and a stimulus which calls out any of these responses as being in the focus of attention or awakeness or awareness.

I shall take as the three chief categories of behaviorist psychology, then (1) stimulus, (2) response, and (3) dominant complex—the group of response-patterns which at any time are on the threshold of readiness to function, if stimulated.

If we take birth as a convenient date at which to make an inventory of the behavior traits of a human organism, we may say that native traits fall largely under three heads. (1) A human being begins life on his own account with certain response-patterns. Some of these are more or less rigid, particularly on the effector side, reflexes. With most of these the pattern of response remains recognizably constant throughout life, although some alterations in their muscular and glandular make-up may take place, and on the receptor side there is the possibility of substitution of one set of receptors for another. (Some reflexes, of course, develop in the postnatal period.) Other native response-patterns are highly modifiable, instincts in the Watsonian sense. (2) The new-born infant has also certain complexes into which these initial response patterns are organized. The clearest case of this is a complex in which relatively few patterns are potentially functioning—a complex which remains of a fairly constant type throughout life, sleep. (3) In addition to patterns and complexes of patterns, the infant organism possesses certain capacities for the development of new patterns and complexes, or on the negative side certain limitations to the adult behavior traits which he is capable of developing. It is under this third caption, capacity, that the chief distinctions between man and other mammals are to be found, and incidentally between idiot, moron, and normal

human being. Some capacities are dependent on gross structural traits, such as the human hand; for others no structural basis is at present clearly identifiable.

While change of behavior traits (response-patterns and complexes), or learning, continues throughout life, the flexibility of traits diminishes with age, and in early childhood some of the most important phases of the growth of the human personality take place. The learning process has a dual aspect. On the one hand it involves the development of new response-patterns, the building up of new combinations of effectors and the breaking down of old ones, and the regrouping of receptors with the various central nervous processes and effector combinations, leading to a more or less minute sensitivity to differences of stimulus. The phase of this process which has received chief attention is the substitution of one set of receptors for another, called conditioning or association,—a new set of receptors and a new or substitute stimulus may come to be associated with a given effector pattern, by repeated presentation of old and new stimuli together.

On the other hand, the learning process involves new combinations of response-patterns into complexes. As a new inhibition develops or an old one disappears, a response-pattern may be taken from or added to a complex. Each of these aspects of the learning process affects the other. The change in form of a response-pattern means alteration in a constituent of various complexes. And the possibility of altering a pattern, *e.g.* by conditioning, depends on whether the new stimulus comes adequately within the range of attention, *i.e.*, whether it sets up an activity of receptor organs and nervous system which may be associated with the other pattern.

So much for our hurried survey of the outlines of a possible behavioristic analysis of human activity. This process of growth of the personality takes place in an environment of which other persons form a most important part. While social groups differ widely from time to time and place to place, there is a feature common to them all which calls for a

word of comment, the *mores*. No known human society is without mores. A mos is a type of response, not necessarily a specific response-pattern, which a given situation calls for. If *S* fails to respond in the socially established way, other members of the community will respond to his failure in ways that have the effect of punishment. How it comes about that all human societies have mores or that any particular society has a particular set of mores, is a question which it is an important part of the institutionalist's task to try to answer. No adequate survey of this problem is possible within the limits of this paper. It involves a detailed inventory of human functional native traits, an application of the natural selection hypothesis to mores as group traits of survival value, and a detailed study of the phylogenetic process of cultural evolution. Suffice it to say that our mores are numerous, and that they prescribe in minute detail much of human activity. There are legal mores, mores of grammar, spelling, etc., mores of politeness and style, mores of sex and family life, mores of friendship and loyalty, mores of leadership and social grouping, professional mores, mores of humor and art, mores of health (physical and mental), mores that we call morals, and mores of reasoning and belief. To the institutionalist, social organization consists of mores. The growth of the human personality during childhood is very largely a process of education or inculcation of the habits and complexes dictated by the mores. For the child, learning is predominantly learning to mind the mores. And in this process we are assuming as an established fact, though one perhaps not adequately accounted for scientifically by present psychology, the efficacy of punishment in learning.

III

We may now pass in hurried review a few sample topics in psychology (and social science). If the outline just completed is tentative, these specific formulations are more tentative still. I hope, however, that they will help to make clearer the behavioristic approach and to illustrate the general type of analysis which is consistent with a behavioristic

formulation of psychological problems. Perhaps they will serve in some measure also to substantiate the view that behaviorism is as complete a psychological view as introspectionism in that it deals with all the phases of human activity that introspectionism does. It is not, of course, complete in the sense of claiming to have achieved a final solution of all psychological problems. It does not *e.g.*, claim to offer a complete and satisfactory scientific description of all telic behavior in non-teleological terms. But a behaviorist can deal with this set of problems at least as satisfactorily as an introspectionist, so far as they accept the same criteria of scientific accomplishment. An introspectionist like McDougall, who takes teleological categories as fundamental, will no doubt have a more complete solution of his problems, but only because his problems are less inclusive.

Perception.—To a behaviorist the term perception is ambiguous. (1) In the broadest sense perception is coextensive with response. Any response to a stimulus involves perception of it. Perception of an object is reception of a stimulus. The perceived object is the stimulus, and the perception of it is the activity of the receptors and afferent nerves. (2) In a somewhat narrower sense perception is the functioning of such a reception-pattern as will enable the organism (person) subsequently to report on or specify the stimulus if asked to do so. (3) In a still narrower sense perception is a response of a symbolic sort,—a response which may serve as a substitute stimulus for the responding organism or for other persons. If it serves as a substitute stimulus for other persons, it commonly conforms to certain established conventions—to the mores of language. A report is precisely such a response. If it is merely a substitute stimulus for the responding organism, it may or may not be a language response—depending on the organism's habits (learned response-patterns).

To a behaviorist the introspectionist's view of perception often involves not only a confusion of these three meanings of the term, but also a confusion of stimulus and response, a 'stimulus error.' In the third of the three senses perception

is a knowing response, and this is the type of knowing response which has received chief attention in the hands of epistemologists, the subject-object relation. For the behaviorist the subject-object relation is not ubiquitous, but is a special case of the stimulus-response relation. The knowledge is the response-pattern, and the known is the stimulus. The common confusion of the older associationist view, which still persists in such forms as the attempt to regard the object (stimulus) as a part or resultant of the psychological functioning of the organism, is well brought out by reversing the Berkeleyan proposition to read *percipere est esse*. For a behaviorist, to perceive an object is not to be it, but to respond to it as a stimulus. The stimulus is the *Ding an sich*.

An illustration may be in order. *P*'s subject, *S*, is driving a car. The light at the corner is red and *S* brings his car to a halt. An appropriate question may bring the response "I had a red sensation." *P* accepts this report, introspection if you will, precisely as he might the report "I saw a ghost." Either report throws an interesting and valuable light upon the language habits of *S*. But *P* reserves the right to infer from the former report a case of stimulus error. It was presumably the stimulus and not *S*'s condition (mental state, if you prefer) that was red, though *S*'s responses may have included one which symbolized the redness of the light *e.g.*, the verbal response, the word 'red.'

It has been alleged that the behaviorist denies images. He does deny that knowledge is made in the image of its object (response in the image of stimulus) except perhaps in the case of sympathetic and imitative responses and of onomatopoetic words. But in the second and third of the senses above assigned to perception a conditioned perceptual response is of course admitted and insisted upon. At least a part of a visual or auditory reception pattern, or a symbolic response, may be called out by some stimulus, internal or external, other than that which arouses the original visual or auditory receptors. Much human thinking presumably involves such conditioned responses.

Desire and Purpose.—Desire and purpose may be taken to

illustrate what I shall call 'telic behavior.' In this connection it is important to distinguish between the terms 'telic' and 'teleological.'[5] The word 'telic' designates behavior in which antecedent responses appear to be determined by the consequent 'end.' The word 'teleological' applies to terms or statements which imply that consequent does determine antecedent in telic behavior. It is sometimes felt that to describe telic behavior in non-teleological terms is to deprive it of its telic character. This is precisely analogous to the theory that to understand man and monkey as descendants of a common ancestor is somehow to dehumanize man. Man is no less man for being known as a primate, nor is a human purpose less purposive for being understood as a special type of animal, and even of bodily behavior in general. Telic behavior itself is not altered by altering the scientist's language behavior about it. The most that can be asserted is that a description of telic behavior in non-teleological terms will inevitably be a wrong description—which is equivalent to assuming that human behavior lacks the uniformity which makes a scientific description of it possible. This may possibly be true; but it cannot be proved a priori, and we are more likely to make progress in anthropological science if we assume as a working hypothesis more uniformity than has yet been discovered. (Article I.)

The task of accounting for telic behavior in non-teleological terms may be formulated as that of understanding such behavior as consisting of stimulus-response sequences. The key to the present attempt to deal with the case of desire and purpose lies in the conception of the dominant complex;—most of the response-patterns of an organism are inhibited, so that only a few are potentially active at any moment.

If a complex includes chiefly responses which happen to be more or less appropriate to bringing about a determinate result, the complex continuing to consist of these responses until the result occurs and then shifting, the behavior of the organism would properly be designated as telic—a series of responses each of which appears as a 'trial' and each but

[5] This distinction was elaborated in Psychol. Rev., 1926, 33, pp. 254 *ff*.

the necessary ones as an 'error.' We may call this type of dominant complex a 'drive.'

The problem of describing drive behavior in non-teleological terms involves accounting for (1) the continuation of a drive complex until the consummatory response occurs or the end is achieved, (2) the shifting of attention when the consummation occurs, (3) the variability of the preparatory responses, (4) the appropriateness of the preparatory responses to bringing about the end-result.

(1) In some cases a continuing stimulus may account for the continuation of the drive-complex, as in the case of a dog on the trail of a fox. But the complex may continue of its own momentum unless attention is shifted by the presence of some new appropriate stimulus;—the dog may lose the trail, and pursuit-responses nevertheless continue. (2) Some responses included in the complex may be inappropriate to the end,—*e.g.*, the end of catching the fox—and if called out may inhibit the pursuit responses, thus shifting the dog's attention. Again, the consummatory response may eliminate the inducing stimulus, *e.g.*, eating may eliminate the arousing condition (empty stomach, etc.) for a food-getting complex. (3) The variability of response is an essential characteristic of most complexes, whether or not of the drive type. The stimuli in the range of attention (foreconscious) call out their appropriate responses one-at-a-time. The several patterns are mutually inhibitory. The functioning of one pattern may change either bodily or external conditions (or both), thus bringing a new stimulus into the focus. (4) The most difficult part of the problem is to account for the inclusion in the complex of responses which are appropriate to bringing about the result on other grounds than their appropriateness as such. If we were to explain the inclusion of these responses in the complex on the basis of their appropriateness we would still be giving a teleological account of telic behavior.

A partial non-teleological account of this inclusion of appropriate response-patterns may be made by an adaptation of a suggestion of Watson's: On previous occasions the shifting of the complex must have required some of these responses so

that they tend to occur on the whole more frequently than inappropriate responses and so to continue in the complex. Once a complex (inhibition of a large group of reaction-patterns) induced by a particular condition has begun to develop, it will tend to continue to function until this condition is removed, and so responses which will remove it tend to occur each time the complex becomes dominant and to become conditioned to stimuli which commonly accompany the inducing stimulus. On the other hand responses not appropriate to eliminating the inducing stimulus need not occur if the complex is to cease dominance, and their less frequent occurrence makes it less likely that they will be conditioned to stimuli which commonly accompany the inducing stimulus. It may further be noted that some inappropriate responses may have become inhibited by conflict with appropriate response-patterns which are included.[6]

This treatment is intended not to be exhaustive, but merely to indicate the nature of a behavioristic account of this complicated problem of drives. It should be noted that a drive may include many definitely 'inappropriate' responses along with more plausible 'trials.'

Thus far we have considered the general case of a drive or desire. Purposive behavior may be conceived as a special case of drive, in which the inducing stimulus for the complex is a language-response which represents or symbolizes the end, or in which the inducing stimulus is a *conditioned* sensory response (image), the 'original' stimulus to which is a part of the end-situation. This inducing stimulus (language response or conditioned sensory response symbolizing the end) is thus the 'conscious purpose.'

Values—Meaning and Beauty.—Only a few comments on these topics are possible in so brief a survey. While evaluation is not properly a part of scientific description, it is a part of the descriptive task of science to describe the type of human activity we call evaluation. It is the essence of the present contention that this task can be only partially

[6] Desire, choice, and purpose from a natural-evolutionary standpoint, PSYCHOL. REV., 1926, 33, pp. 257–8.

accomplished by psychology, *i.e.*, by a study of the individual. Psychology requires to be complemented by a study of group behavior.

Upon the psychological side it is to be noted that the value of an object differs from its physical or observable properties in something like the way in which secondary and primary qualities were once supposed to differ. The value of an object is peculiarly dependent upon the response-patterns and complexes of the particular person who happens to be evaluating it. Its value may mean its capacity for calling out a particular type of response; *e.g.*, it is funny if it calls forth a smile or laugh. Or it may mean that it plays a particular part in the type of complex called a drive, *i.e.*, it is the condition or event which eliminates the inducing stimulus.

But the psychological side is not the whole story. If it were, the behaviorist-institutionalist would be a mere sophist. There is a certain objectivity about beauty and funniness. To say that the mores prescribe in some detail the modes of action of an individual and compel him to develop certain habits and drives is to say that within limits they determine what he shall laugh at and what he shall appreciate æsthetically. In the case of the humorous object, the form of the response is largely determined by heredity, it is a laugh or a smile reflex. But even here the mores are a factor in determining which of these reflexes shall be set off, and in a measure also in modifying the pattern of the reflex itself,—*e.g.* a laugh may be either raucous or refined. In the case of beauty, the mores are largely influential both in determining what objects are to call forth an esthetic appreciation, and in determining what type of response constitutes appreciation. Native traits affect artistic activity chiefly in that the activity prescribed by the mores of art appreciation and creation may exceed the capacity for discrimination and performance of many organisms that pass as normal persons.

Meaning, properly speaking, is a value attribute not of objects in general, but of that specific class of objects or stimuli which comprise language behavior and other symbolic responses. The meaning of such a symbol or combination

of symbols is in part the object or group of objects for which it is a symbol or substitute-stimulus, and in part its appropriateness in a given context of other symbols, *i.e.* its appropriateness as a response to other symbols or as a stimulus to other symbols. It will doubtless be felt that this makes meaning a very complicated affair. So it is. Language is an extraordinarily complicated institution, consisting of an elaborate and minute set of mores of grammar, diction, rhetoric, punctuation, spelling, pronunciation, vocal inflection, and logical sequence. Meaning, so far as language activities go, is closely prescribed by these—in the English language—easily a million mores.

Knowing Responses and Thinking.—Thinking is a type of activity that involves the functioning of knowing or symbolic responses. The knowing responses are not necessarily verbal; nor are they necessarily confined to the functioning of the nervous system, the vocal apparatus and conditioned reception-patterns; they may include readily observable behavior of a non-vocal sort: tool-manipulation responses, such as drawing or writing on a paper. Not all functioning of symbolic responses, however, constitutes thinking. Thinking may be conceived as the working out of a drive in symbolic form—a symbolic process of trial and error, almost literally 'making one's head save one's heels.' The inducing situation to the drive may be put symbolically as a question. If the answer is not a verbal perception and is not a habitual language response (memory), a drive may be induced in which the answer is worked out by a process of trial and error. This working out, the symbolic trials, is the thinking; and the response which eliminates the inducing stimulus is the 'answer.' Whether or not the question arouses this type of activity depends upon the language habits of the subject and the complex which happens to be dominant when the question is put.

Phylogenetically it seems appropriate to conceive this thinking process as originating under conditions where the inducing stimulus is concretely present in something more than symbolic form and where the answer is immediately translated

into overt, non-symbolic action. But this does not require, as some Pragmatists have seemed to say, that thinking should always eventuate in overt action; thinking may become a game pursued for its own sake, as in cross-word puzzles, or in science.

Much of human thinking proceeds largely in the form of language behavior. So far as this is the case the process tends to conform to the mores of which language consists. The question is often derived from social inheritance, *i.e.* from the mores. The terms in which the thinking proceeds are largely determined by the mores of language, *i.e.* the categories of thought, the methods of analyzing and classifying things, come largely from the mores. Many truths are ex-cathedra or more strictly ex-more truths. And the sequences which are accepted as 'logical' are the sequences which the mores prescribe. Upon this aspect of human thinking rests its objectivity, rests the possibility of persuading others by argument. Thinking in a manner which has more than individual validity is as much conforming to the mores as behaving politely.

Pain, Fear and Anger.—We may indicate briefly the nature of a behavioristic conception of emotional activity by a few comments on these three types. They have in common an activity of the adrenal glands, inhibition of digestive activity, an increased activity of the heart and respiratory system, and to some extent a heightening of overt bodily activity. And typically they share with certain other forms of emotional activity, weeping and laughing, an inhibition of the type of activity just discussed, thinking. Hence, no doubt, the common sense opposition of reason and emotion.

Of the three, only pain is clearly an activity of the drive type. For a behaviorist pain is the stimulation of the specific pain receptors. The ensuing nerve impulses commonly take precedence over others. Pain responses are part of nearly every dominant complex. Typically they include activities of major skeletal muscles, movements of the stimulated part, or touching it, and general bodily movements such as walking the floor. In some cases the result is removal of the inducing

stimulus, as when one comes in contact with a hot object, and moves away. In some cases the drive fails to achieve a consummatory response. It should be noted that certain gesture responses (responses, the function of which is to stimulate other persons) are often incorporated in the pain complex, the groan reflex and facial contortion reflexes.

A fear complex may also include vocal and facial gesture-reflexes. Sometimes it involves trembling and a somewhat general paralysis of overt activity—sometimes a heightened overt activity. Suddenness is a frequent characteristic of the stimulus, but the appropriateness of a situation or event as a stimulus to fear behavior is largely a matter of individual habit formation.

Anger is not so much a drive as a potential phase of any drive. When a drive continues for a time without a consummatory response, the organism gets, as we say, 'out of patience.' Tone quality of voice, flashing eyes, the set of the jaw and facial muscles are common observable features of such a condition. The grosser movements become quicker and more violent. Muscular coördinations are less nice and precise. But the concrete reaction-patterns that function are for the most part a matter of the individual's habits. To this it must be added that in anger, and in pain and fear as well, the mores are an important factor both in determining the habits the organism possesses, and in determining which habits and reflexes are appropriate to the occasion. It is a common current practice to resort to oaths, when 'out of patience.' And our language affords a special part of speech appropriate to emotional conduct.

Scientific Observation.—Because of its peculiar importance for the issue between the behaviorist and the introspectionist, a closing word may be said about the type of human activity we call 'scientific observation.' To the institutionalist science is an esoteric cult comparable to the monastic cult of the medieval Catholic Church. Its ritual and its faith are different, but they are no less truly ritual and faith. Scientific observation is part of the cult ritual and its revelations are part of the cult faith. A scientific observation is, of course,

a special case of perception in our third sense, verbal perception, a symbolic verbal response to a stimulus. But to be scientific the response must conform to the mores of science. Rigorous training is necessary to acquire this type of language habit. To be scientific the verbal response must employ certain types of categories of thought, certain methods of classifying objects. Scientific observation differs from the verbal perceptions of primitive man, *e.g.*, in that it does not classify fire, weather, rivers, and mammals together as animate beings. Again, to be scientific, an observation must be what the scientist regards as descriptive. "It is a beautiful day" is doubtless a frequent observation, but clearly not a scientific one. It evaluates rather than describes. (See Article IV.) It is of the essence of the present view of perception that it is selective. It is selective not only because the dominant complex determines to which of the stimuli for which the scientist possesses correlative response-patterns he will respond. It is selective also because the response-patterns he has developed determine what differences in the stimulus situation can call out different verbal perceptions. Many verbal perceptions, verbal responses to many aspects of a situation, are doubtless possible that would not class as scientific. To be scientific an observation must have relevance to some part of the scientific faith in which the observer is concerned as one of the high-priests, it must have relevance to a scientific hypothesis. There is nothing inevitable about the truth of a scientific observation. It is true merely because the mores of science prescribe belief in those revelations which are made according to the ritual of scientific observation. Observations are true not because they are logically deducible, but because they conform to observational ritual. They are the dogmas of the scientific cult. If scientific observation is thus a matter of language-habits and the mores, a product of social evolution which itself has evolved and is evolving, we might expect occasional conflicts within the esoteric cult of science as to what constitutes a scientific observation. And this is precisely what we find today. Are observations in which there is not the possibility

of substitution of one observer for another on the same plane of scientific validity as those in which substitution is possible? Does one observe external stimuli, or is the object of observation a phase or resultant of the psychological functions of the organism? Are scientific observations mere language habits and cult ritual? To some it may seem that such an attempt as this last question suggests,—an attempt by social science and psychology to study science scientifically—to deal with science as a product of social evolution—is to call in question the validity of the scientific faith. But to understand that the basis of scientific truth is sociologically and psychologically similar to that of religious truth is not to give up the scientific faith, but to follow it to its logical conclusion. The mores of scientific observation are no less current mores for being understood as such. And as mores they are enforced by a penalty. The penalty for unbelief in the revelations of scientific observation, like the penalty for unbelief of the medieval cult, is excommunication. As a devout mos-fearing behaviorist I subscribe to the scientific creed, respect the ritual of observation, and claim membership in the cult of science.

Summary

The chief issues in the anthropological studies today are between those who adhere rigorously to a scientific approach and those who do not. Important canons of science and findings of geology and general biology require the treatment of psychology and social science as branches of biology, and as sub-branches of physical science, and treatment of man as a special case of a metazoan organism and of a physical body. Fundamental categories of psychology are stimulus, response, inhibition, and dominant complex of response-patterns. The sociological concept of *mos* is essential to an understanding of individual behavior. 'Unanalyzable wholes,' 'emergents' and unanalyzable purposes are unscientific categories. Introspectionism offers nothing of scientific value not incorporated in behaviorism, and often involves a stimulus error in its treatment of perception. A suggested scientific formulation

of psychological problems includes: (1) desire and purpose as special cases of dominant complex; (2) value as an attribute of an object qua stimulus, dependent on individual habits and group mores; (3) thinking as a special type of purposive behavior in which the preparatory responses are symbolic and dependent on individual habits and group mores; (4) emotional conduct as including certain specific inhibitions, certain specific gesture-reflexes and certain conventional language-responses; (5) the subject-object relation of epistemology as a special case of the stimulus-response relation, involving a verbal perception response; and scientific observation as a subclass of verbal perception, defined by the mores of the scientific cult.

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ECLECTICISM VERSUS SYSTEM-MAKING IN PSYCHOLOGY

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A detached survey of current system-making in psychology is apt to prove a discouraging procedure if illuminating insight into the nature of the science be the object of such a survey. The personal antagonisms, charges and counter-charges, enunciation of 'new' principles, promulgation of basic axioms, contempt for rival systems make for an intellectual atmosphere where the amount of light produced seems to be inversely proportional to the amount of heat engendered. Such a confused, acrimonious atmosphere is conducive neither to clear thinking nor to psychological progress. Under the circumstances it might be pertinent to ask whether the trouble is with psychology or with the system-makers. Another way to formulate the question is to inquire why we have *systems* of psychology but no *systems* of chemistry, physics, astronomy, or physiology? May it be that this phenomenon is indicative of a still active philosophical trend among psychologists,—a trend that is anachronistic among the natural sciences? At all events it appears significant to note how appropriate it sounds to ask a philosopher or a psychologist what system he espouses and how ridiculous it would sound to ask an astronomer or chemist a similar question.

At the present time we have enough systems to establish a rather imposing psychological department store. To consider the main floor only: one counter doing an active business is presided over by a *Gestaltist;* the adjacent one is limited to behavioristic goods of a style that is either radical or conservative depending on the psychologists in charge. Further over we find a counter devoted exclusively to redintegrative patterns guaranteed to clothe adequately all forms of psychological nakedness. Beyond that, somewhat neg-

lected and with a poor sales record in recent years, is the once flourishing associationist counter. The uninitiated consumer is thrown into a welter of harassed confusion by placards of contradictory advertising appeals. He is urged in the one case to avoid associationist goods because of its shoddy atomistic weave, a mere bundle of questionable fibres. The same placard warns him of the dangers of being misled by the handsome clerks behind the behavioristic counter whose patterns are constituted of a quickly disintegrating chain of reflexes. Instead he is implored to deal exclusively at the *Gestalt* counter where every unit of goods is a genuine unit and where the whole amount he purchases will be more than the sum of the individual pieces purchased. In another case a placard will advertise the psychological dispensation that will be his if only he will invest exclusively in the 'electron-proton aggregate' brand of behaviorism, guaranteed to be free from ancient adulterants in the way of toxic conscious stuff. Another placard carries the appeal to follow the prevailing Russian mode of having all psychological material properly conditioned, or if necessary, re-conditioned.

Should our worried consumer in his despair wander down into the bargain basement, he will be confronted with an even more disordered scene. Here he will find psychoanalytic counters piled high with subconscious and unconscious symbols, complexes, Ego instincts and souvenirs of his forgotten loves. He might even find bargains in hormic principles, brain centers that function in some parts with no part guaranteed, miracle-working endocrine glands, a complete assortment of sensations, images and simple feelings of a standardized structure but not much function, and maybe quite a number of functions with no standardized structures. There are of course other counters and even other floors he might visit, but for our purposes there is no need to prolong his bewilderment. It should be obvious that the business needs re-organization, more internal coöperation and an elimination of friction.

The primary evil of system-making, as we see it, is its tendency either to ignore or to distort facts that fail to fit

into the system. Occasionally a fact may even be denied because of its refractory character with reference to available openings in the system. Once a psychologist creates a system or affiliates himself with the creation of a fellow-psychologist he becomes a partisan. Such partisanship is likely to exert a pernicious effect on his subsequent psychologizing. It functions as a mental set, facilitating consideration of phenomena in line with the system and inhibiting judicious evaluation of phenomena running counter to the implications of the system. To make the system paramount and facts subsidiary is the pernicious consequence of partisan loyalty. It is the *modus operandi* of the zealous politician, the leader of a cult, and the enthusiastic but unenlightened fundamentalist in theology. Political systems, therapeutic systems, and theological systems are the inevitable result. It is important to note that the result is not *science*, in any acceptable sense of the term. We fail to find republican, democratic and socialistic sciences. Nor do we find *sciences* of vegetarianism, Couéism, chiropractic, Volsteadism, Mohammedanism or Voodooism. At the risk of indulging in a platitude we venture to emphasize the patent but frequently neglected principle that healthy scientific progress requires loyalty to facts rather than to a given system.

The present-day procedure in psychology seems to involve the adoption of a 'basic' explanatory or descriptive principle which is then used as the foundation of a system. This is somewhat of an exaggeration, of course, but a legitimate one for expository purposes. Accordingly in one text we find *all* psychological phenomena squeezed into the mold of conditioned reflexes and whether they are distorted in the process or not seems immaterial provided no other mold has to be employed. Impressed with the unique characteristics of perceptual configurations the *Gestaltists* proceed to apply the configurational principle to *all* mental phenomena with almost a contemptuous neglect of other principles that might prove more adequate in given cases. The system *qua* system becomes all-important. "If it isn't a Gestalt it's poor psychology," becomes the slogan. A sort of all-or-none law

seems to activate the system makers: if a principle is to be adjudged acceptable, for some mysterious reason it must be applicable to the entire gamut of psychological phenomena. A given phenomenon is 'explained' in as many different ways as there are available 'systems.' Whether the burnt child avoids the candle because he *associates* the visible flame with the memory of the pain, or because the flexion reflex has been *conditioned* to respond to the photic stimulus, or because a fragment of an antecedent pattern suffices to *redintegrate* the consequent of the original complete pattern, or because of the potency of the scorched-finger *configuration*, varies not so much with the child as with the psychological texts. Kindred individual differences among the rival psychologists are revealed when we turn to almost any of the customary problems of learning, intelligence, perception, or thinking. The only exceptions seem to be in those cases where the interests of the system have rendered it advisable to legislate a troublesome phenomenon out of existence by a fiat of the system-maker. For example, a given system-maker refuses to consider the topic of imagination because to do so might necessitate the recognition of visual images and such recognition would destroy the symmetry and logical consistency of his system. Under the circumstances, he does one of two things. He either repudiates the reliability of the observer's report and denies the occurrence of such a phenomenon, or else he selects such phases of the report as can be salvaged to fit the system, and ignores the other phases. This may be excellent system-making but it is poor scientific method.

By way of contrast it is advisable to consider the methodology of such an established science as physics. We do not find a medley of 'systems' of physics, one based exclusively on the laws of hydrodynamics, another on Ohm's law, a third on the laws of refraction, and a fourth on the laws of thermodynamics. Each law, principle, or explanatory formula is utilized in its relevant context. It is true that the theorist in physics in his metaphysical moments is pre-occupied with electron-proton vagaries and energy quanta, but he doesn't permit such pre-occupation to shape his consideration of

physical phenomena in connection with *every* topic. A Sabine studying problems of sound applies principles of acoustics to account for variations in the phenomena observed and doesn't strive to fit his facts into some particular electron-proton system. The wave theory of light is not employed to account for water waves. Archimedes' principle is not made the key to all physical problems. Pascal's law is not made basis of a complete system of physics by endeavoring to apply it to electrical, magnetic and sound phenomena. Its sphere of influence is limited to the phenomena to which it applies. There is no need to labor so obvious a point. A little reflection will show that what is here said with reference to physics applies just as accurately to chemistry, physiology, botany and astronomy. We have in each case several fundamental principles or laws to deal with the particular groups of phenomena constituting the science in question. We do not find separate 'systems' of each science growing up around one key-principle or one master-law.

In all fairness it should be recognized that the intensive utilization of a single principle of explanation has made for psychological progress despite the confusion engendered. The processes of ideational association, of conditioning, of configuration, of redintegration, of neural functioning, and of instinctive activity have all been widely explored and been the means of increasing the sum total of our factual knowledge. It is only the endeavor to push one process to the exclusion of the others that is responsible for the friction in the psychological household. In the endeavor to sponsor the virtues of a specific principle there has even been occasional neglect of accomplishment by adherents of a rival principle. An example of this is to be found in the *Gestalt* doctrine of the uniqueness of each configuration in terms of its pattern or organization.[1] By way of emphasizing the novelty of this

[1] It may not be amiss to point out that the recent *Gestalt* polemic against the atomistic nature of pre-*Gestalt* psychology may not be as novel or as deserved as the more ardent disciples seem to think. In discussing the differences between the older and the newer sensationalism Titchener it will be recalled, presents the following argument:

"Meanings are stable, and may be discussed without reference to time; so that a psychology whose elements are meanings is an atomistic psychology; the elements join,

doctrine it is pointed out that contrary to the teachings of the associationists, such uniqueness cannot be attributed to a summation of the constituent parts of the *Gestalt*. And yet both Tucker [2] and the younger Mill [3] had stressed this phenomenon, the former under the concept of *fusion* and the latter under the term *mental chemistry*. Even Hartley had not neglected it. In other words, this is just as much an associationist contribution to psychology as a *Gestalt* contribution. Furthermore, in this connection, it might be appropriate to ask just how this phase of *Gestalt* teaching differs from Wundt's concept of creative synthesis.[4] In fact, long before the rise of *Gestalt* psychology Wundt's pupils had demonstrated the configurational principle in the course of their tachistoscopic experiments on reading. These casual historic references are not by way of detracting from the value of the *original* contributions of the *Gestalt* psychologists, but merely to indicate the continuity of psychological development and the futility of enhancing one explanatory principle by deprecating another. It is the latter tendency that seems to be the bane of system-makers.

The upshot of such system-making is that explanatory principles are presented as mutually exclusive and the student is confronted with an array of *psychologies* instead of a single science of psychology. In fact, the shifts in reigning systems seems to require chronological description to keep the records straight. In this connection, to revert to an earlier point, we might ask why we have Psychologies of 1925 or 1930 and

like blocks of mosaic, to give static formations, or connect, like the links of a chain, to give discrete series. But experience is continuous and a function of time; so that a psychology whose elements are sensations, in the modern sense of the term, is a process-psychology, innocent both of *mosaic* and of *concatenation*. This is a point which Wundt, the father of experimental psychology, never tires of emphasizing." (Italics ours.) Lectures on the experimental psychology of the thought-processes, 1909, pp. 26–27.

[2] H. C. Warren, A history of association psychology, 1921, p. 66.

[3] *Ibid.*, pp. 165–166.

[4] The following passage, in which Wundt is referring to 'the principle of creative resultants,' might have been culled from the text of an orthodox *Gestalt* psychologist:

"It attempts to state the fact that in all psychical combinations the product is not a mere sum of the separate elements that compose such combinations, but that it represents a new creation." An introduction to psychology (translated by Pintner), 1912, p. 164.

no Chemistries or Astronomies of 1925 or 1930? As we see it, the anomaly would cease to exist once it were recognized that these principles of explanation are neither mutually exclusive nor intrinsically contradictory. Both the principles of conditioning and those of ideational association are rooted in the factor of contiguity. If a rigid distinction must be made, it might be well to reserve the latter explanatory principle for cases involving obvious ideational processes and the former for cases apparently devoid of such processes. The distinction may be more a matter of degree than of mechanism. Hamel's study [5] of conditioning suggests the presence of fleeting ideation as an effective constituent of even orthodox types of conditioned reactions, and the studies of the Würzburg laboratory reveal non-ideational control-processes in the routine associative sequence. There is no need to debate the relative merits of the two principles on the basis of which is the more enlightening principle. They are equally serviceable as descriptive or explanatory formulæ, and the adoption of the one does not necessitate the rejection of the other.

By way of specific illustration we might point out that the course of events characteristic of a commonplace revery are best treated in terms of association, while the well-known cases of neurotic fears for which the sufferer can furnish no clarifying nexus are best described in terms of conditioning. Similarly, where the outstanding characteristic of a given mental phenomenon is the substitutive and fragmentary nature of the effective stimulus, it might be well to employ *redintegration* as the explanatory concept. This concept stresses a very real and important phase of psychological events and the term is decidedly apt as a descriptive device. In other words, whenever it is desirable to point out the efficacy with which a small portion of a previous whole now functions in place of the antecedent in its original completeness, the term redintegration serves admirably. But it too should not be regarded as necessarily eliminating the need for or the desirability of other terms to describe other phases of mental life. For instance the term *Gestalt* or configuration is

[5] I. A. Hamel, A study and analysis of the conditioned reflex (Psychological Studies from Catholic University of America), Psychol. Monog., 1919, 27, no. 118.

most helpful to symbolize those integrated experiences which prior to 1912 were referred to as stimulus-situations or perceptions.

However, such recognition of utility does not obligate one to scrap every other principle in a spirit of monogamous psychological loyalty. When the explanation of a given problem focusses around the *organized* character of a stimulus-pattern or the *integrated* nature of a response, the principle of *Gestalt* represents a splendid contribution. In connection with other problems whose explanation requires analysis of the total flux of mental processes it may be more advisable to neglect configurations and refer to sensation, images, or feelings. Occasionally reference to neural functions or other physiological and anatomic features may be more relevant as explanatory factors than any of the others. At such times the psychologists ought not to permit allegiance to a specific system to blind them to such relevance.

This entire issue can be summed up adequately by taking note of a specific problem upon which much light has been shed, such as that of space-perception. It would be somewhat absurd to venture to reduce this problem to a *single* category of explanation. A complete account of the parts played by all the known factors operative here would require consideration of every one of the explanatory principles mentioned. To make the phenomena in question exclusively a configurational affair, or solely a matter of redintegration, or wholly a mere linkage of the retina to occipital tissue, or predominantly a matter of ideational associations would be a ridiculous performance. The result would be either an incomplete account of space perception or a very much distorted one. Some aspects of such perception are best accounted for in terms of one principle and other aspects require different principles. The task of the psychologist is to assemble the data and apply relevant principles to the appropriate aspects, recognizing that these principles are supplementary and not contradictory. Failure to recognize this, results in pernicious system-building in which one principle is over-worked to explain events that would be more clearly and simply ac-

counted for in terms of one of the rejected principles. What is even worse, the system-builder tends to ignore facts at variance with his favorite principle, or else in Procrustean fashion he mangles them to fit the system. Might it not be preferable in the interests of psychological progress, if a choice has to be made, to mangle the system and preserve the integrity of the facts, or better yet, give up systems entirely, and have a single science of psychology? Judicious eclecticism in the choice and application of explanatory principles even if this means an unfinished science, seems to us to be preferable to partisan attachment to a single principle in the interests of a *finished system.*

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LEARNING AND THE LIVING SYSTEM

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The purpose of this paper is to offer an analysis of certain implications of the current term 'living system,' and to show that learning may be described by the use of modern physical terms as the capacity possessed by the more complex of such systems of making an adjustment to a four-dimensional situation of a certain nature. The view put forward presupposes a modified form of stimulus-response psychology, according to which theory behaviour is an adjustment made by the organism to a stimulus or complex of stimuli, stimulus being defined as 'any alteration in the external vital conditions.' It will be maintained hereafter that learning is an extension of this capacity to adjust, differing from it in degree, not in kind, in that it involves the capacity to make an adjustment to a four-dimensional situation-manifold including such stimuli or stimulus-complexes. It will be realized that this conception offers no explanation of the nature of learning, any more than does the stimulus-response hypothesis offer an explanation of the nature of stimulation.

Before considering the implications of the term 'living system' a brief discussion will be given of the use of the term 'system' as applied to inorganic complexes. "Any material system is regarded from the dynamical point of view as constituted of a number of particles subject to interconnections and constraints of various kinds" (**28**, p. 32). In this definition two points are stressed. First of all, the system is a complex. It comprises a number of elements. Secondly, these elements are not entirely discrete, but are interrelated so that they form the unity which is the system. Physical analysis determines and expresses in an invariant form the properties of the parts of such systems as contrasted with the whole, and the properties of the whole as contrasted

with those of the parts. Thus a pendulum swinging in air forms a simple mechanical system, and it is possible to determine the rate of motion of the centre of gravity, which is a property of the whole, or the tension in the supporting rod, which is a property of a part. In the same way we may have what are called statistical systems, such as a mixture of hydrogen and oxygen at a high temperature. Here reversible combination takes place according to the formula,

$$2H_2 + O_2 \rightleftarrows 2H_2O + 58.3 \text{ Cal. (Lotka)},$$

until, at any particular high temperature, equilibrium is reached. Here Le Chatelier's rule operates (**20**). Increase in temperature for example is followed by a shift in the reaction in the direction absorbing heat, and vice versa. Thus, when the temperature is changed, there are initiated by the system compensatory processes which tend to maintain it at the same temperature. Now this tendency towards constancy of temperature, in spite of disturbance of equilibrium, is a property of the whole aggregate, and may be contrasted with properties belonging to individual molecules as such. It depends upon the fact that the particular circumstances,—in an elementary way the organization,—of the system is such that, given the change of conditioning factor, the requisite compensatory changes of the system follow. In each case, that of the mechanical and the statistical system, we have an interconnected aggregate whose internal conditions and relations cause it to react as a unity towards disturbances of external origin. The pendulum when subjected to a disturbance within certain limits oscillates as a pendulum preserving the relationships which make it a pendulum. If the disturbance is too great, the system is disrupted and the pendulum-properties no longer belong to those particular portions of matter. As in the case of the chemical equilibrium there is a tendency towards constancy in the face of disturbance, this property being due in each case to an organization of a specific nature. From the evolutionary point of view, Le Chatelier made a contribution of fundamental importance when he pointed out that there

may arise in inorganic nature physico-chemical complexes which will exhibit this property in a manner impossible for mechanical systems.

It will be seen that such a property implies a contrast between change of one element or condition of the complex and the compensatory changes of the whole system. Thus when the pendulum bob is struck, compensatory intermolecular forces prevent the bob and the rod from parting company. If the rod were made of brittle material and movement were not possible, the integrity of the pendulum would not be maintained. In the same way, if the temperature parameter of the chemical equilibrium obeying Le Chatelier's law is changed, systemic changes involving the *whole* aggregate must take place in the compensatory direction. Whatever be the complexity of the aggregate in question, the same rule must apply. If such an aggregate exhibits a tendency towards constancy in the face of disturbance of any specific kind, it must be so organized that it can as a unified whole effect the requisite compensatory changes. Thus, there exist inorganic complexes, systems, both mechanical and statistical, that tend to maintain certain states in the face of disturbance of external origin: such complexes are of varying degrees of complexity, and different mechanisms may serve to bring about the requisite compensatory systemic changes.

Now as interconnected wholes, living organisms have many properties in common with such inorganic systems. Before discussing this point, however, a brief notice will be made of the use of the term 'system' as applied to living creatures. Apparently the usage is first to be found in the Greek author, Dionysius of Halicarnassus (**14a**), who speaks, about 30 B.C., of 'the whole system of the body.' In 1729 Bishop Butler (3**a**) says 'the body is a system.' In the early nineteenth century the term was employed by Cuvier (**8a**) and the French anatomist, Vicq d'Azyr (**26a**). Classical examples later in the same century are the works of Lotze (**17**) and Avenarius (**2**), each of which writers makes extensive use of the concept of the interconnected system to describe

organic phenomena. Among more modern writers are Lotka (**16**), who speaks passim of 'biological systems,' Rignano (**23**), who states that "every organism is a physiological system in a stationary condition," and Bayliss (**3**), who tells us that "the cells of these beings [*i.e.* living organisms] are heterogeneous systems" of the type analysed by Willard Gibbs. The same author refers to the amœba as a system. Verworn speaks of 'vital systems' (**26**), Pavlov (**21**) of a 'definitely circumscribed material system,' Child (**6**) uses a similar terminology. Cohen-Kysper (**8**) devotes a book to the consideration of the organism as a living system. These examples are chosen almost at random from the great mass of literature in which the term has been used. This usage is often unanalysed, but is justified, having regard for the use of the term in physical science. This I hope to show in the sequel. One of the most characteristic properties of the organism is its separateness from the outside world. "Like the inorganic system (Maxwell, **19**, p. 3) it has very definitely an 'inside' and an 'outside.'" This is recognised both in popular thought and scientifically in the phrase 'organism and environment,' and although it is often hard to draw the dividing line, yet the distinction is undoubtedly valid.[1] This separateness is achieved in spite of a continual interchange of matter and energy with the outside world, because what persists is independent of the particularity of the matter involved. Closely connected is the notion of the individuality of the organism as such. It is a 'whole,' a 'unity,' an 'individual'; the point has been elaborated by writers of the 'organismic' school of psychology, as, for example, Ritter (**24**) and by physiologists such as Sherrington. The wholeness is possible because the organism is 'structured,' consisting of interconnected elements. "The maintenance of the integrity of the organism as a whole is the elementary function of the nervous system" (Coghill, **7**, p. 90). In this 'separateness' and 'wholeness' there is a close parallel between the organic and the inorganic system. Mention should be made here of

[1] The relation being such that the organism cannot exist without an environment, the two being however logically separable. A classical example of a similar relationship is Aristotle's concave and convex side of a curved line.

the criteria set forward by the school of the Gestalt, viz., that the whole is transposible, and cannot be built up out of its parts, and further, that the parts are altered by inclusion in the whole. These criteria were worked out for statical systems, and have both organic and inorganic application. They may be shown to apply to organisms considered as vital systems,—space, however, forbidding such an exposition in this context.

Most striking, perhaps, to the psychologist is the fact that the organism exhibits a multiplicity of states which tend to be maintained in spite of various changes making for their disturbance. There is first the general organic identity of the creature in question. "Biology must take as the fundamental working hypothesis the assumption that the organic identity of a living organism actively maintains itself in the midst of changing external circumstances" (Haldane, **10**, p. 391). This property of active maintenance of structure-pattern extends even to protoplasm. "We can characterize protoplasm as a regulated chemical system of such a kind that disturbance of the normal structure or composition at once determines constructive and reparative processes that tend to restore the normal condition" (Lillie, **15**). In addition to the general organic identity there is the mechanical equilibrium of the motor system. When this is disturbed by a change in the 'external' conditions, a 'motor reaction' takes place, which, in a simple form, is called a reflex. This is a particular case of Verworn's thesis (**26**) for whom a 'vital system' will be in equilibrium until its vital states are altered by 'any change in the external vital conditions'—which is his definition of a stimulus. An important case of the maintenance of the state of mechanical equilibrium will be considered later. But, as Verworn pointed out, the biological system may be disturbed by changes from within also. In this connection a brilliant paper by W. B. Cannon (**4**) is of fundamental importance. He shows that the body is so organized as to maintain a number of internal constants, such as the quantity of water, oxygen, glucose, protein, fat, etc. This maintenance of physiological constants he calls 'homeostasis,' and his

theory is a development of Claude Bernard's dictum, which he quotes: "It is the fixity of the 'milieu interieur' which is the condition of free and independent life . . . all the vital mechanisms, however varied they may be, have only one object, that of preserving constant the conditions of life in the internal environment" (4, p. 400). Now Cannon points out that "In an open system, such as our bodies represent, compounded of unstable material and subjected continually to disturbing conditions, constancy is in itself evidence that agencies are acting, or ready to act, to maintain this constancy." One of the agencies ultimately at the organism's disposal for this purpose is locomotion of the entire system. Thus "Thirst . . . become[s] more intense as the disturbance of homeostasis is more pronounced and subside[s] promptly when the disturbance is relieved," and under natural conditions thirst is ordinarily followed by locomotion which ceases when water is found. In the same way, protein deficiency is followed ultimately by locomotion. Subjectively, in man, hunger is ordinarily felt. This, as is well known, is correlated with contraction of the stomach, which again is either regulated or directly initiated by hæmic stimulation (11). According to Carlson, hunger acts by increasing the excitability of the organism to external stimulation. This means ultimately that depletion of the food reserves disturbs the equilibrium of the motor system with the environment, and movement follows until food is attained, the baby first finding food 'as a matter of individual experience' (5, p. 10). Thus the food-search is one of the means by which the 'milieu intérieur,' with the homeostatic factors, are kept uniform.[2] Thus we have an objective basis for the powerful 'drives' of hunger and thirst, and for many phe-

[2] It is to be noted that the organic systems responsible for maintaining the homeostatic constants, such as temperature, are not to be thought of as independent, but as forming a parallel to what Hertz has called in mechanics 'partial' or 'unfree' systems, as contrasted with the free system of which they form a part. In certain cases such partial systems may vary in a way not allowable for free systems when it may be said that in certain respects they are subordinate to the total system of which they form a part. See Hertz, 'Principles of mechanics,' Tr., New York, 1899, p. 178, seq. Thus, under certain circumstances, the bodily temperature will rise, according to the conditions obtaining in the rest of the body.

nomena which are subjectively classified under the heading of 'purposive'; for it will be noted that when 'hunger' is felt, movement goes on until food in general, salt, or whatever it may be, is found. As to the sex drive, little is as yet known about its physiology. But it is probably of central origin (Dumas, 9), periodical changes apparently taking place, at least in the female, that sensitize the organism to certain outside stimuli, which thus become effective to cause locomotion until a certain end-state is attained. In any case, activities are here again initiated that cease only when an end state is reached, the disturbing factor being neutralized in the process.[3]

We have seen that the mechanical equilibrium of an organism with its environment may be disturbed by either internal[4] or external change of conditions, the conception of internal change which results in action directed towards the establishment of some end-state thus supplementing the straight stimulus-response psychology. Suppose now that there occurs in the environmental conditions a series of changes at regular temporal intervals, internal conditions being considered as constant. For example, the cage containing a number of frogs is tapped at intervals of about two seconds. The equilibrium of the animals' motor system with the environment is disturbed, and the frogs 'react' by jumping. If, however, the process is repeated, reaction soon ceases. 'Habituation' or 'negative adaptation' has taken place. Here, in the total series of taps, is a change in external conditions more extensive than the first. Considering this more inclusive situation, we see that it has likewise caused a disturbance of the mechanical equilibrium of the organism, which equilibrium is, however, later reëstablished. We have, in fact, in the frog, a biological system so organized that on

[3] Cohen-Kysper, 1914, Rignano, 1926, and Raup, 1926, have all explained organic action as the reëstablishment of a disturbed equilibrium. Weiss, 1925, adopting the same general position, has used the limiting property of the established equilibrium as the basis of an experimental study of Vanessa Antiopa, calculated results agreeing remarkably well with observation. Lewin, 1926, uses the conception of a psychophysical system, thereby raising problems which the present writer has attempted to avoid. See references 8, 23, 22, 27, 14.

[4] Internal to the organism, external to the 'muscular system.'

disturbance of this state, namely, zero motion with reference to the environment, processes are internally initiated which tend to rëestablish the state in question.[5] It must be remembered that in the view of modern physics, the frog is a four-dimensional manifold in a four-dimensional environment, so that we are equally entitled to call the temporally extended series of taps a single situation as to give this term to a spatially extended simultaneous complex of stimuli. Considering then the series of taps as a single four-dimensional situation-complex, negative adaptation appears as an organic adjustment involving the establishment of equilibrium to a temporally discrete four-dimensional situation, this result being effected by conservative processes initiated by the living system. Granting the hypothesis of a living system of sufficient complexity, and bearing in mind the fact that modern physics makes no fundamental difference between the space and the time coördinates, this result might have been predicted. Whether the process be called learning or not is here immaterial; at least it exhibits 'modification' of reaction as the result of previous experience. It seems to present difficulties not raised by a single situation-response relation because we of this generation are involved in Mid-Victorian physical thought, which separates time from space and thus tends to think of a situation as primarily a spatial complex. If we consider the total response to the four-dimensional complex twenty-taps-at-the-top-of-the-box, no qualitative difference appears between this case of negative adaptation and such a reaction as the series of movements employed in eye-fixations. It hardly seems, even, that a more complex organic structure is required for such negative adaptation to take place, for it is well known that the latter occurs at the lowest rung of the animal kingdom, among the unicellulars

[5] It will be realized that a biological system is provided with energy reserves, which, in the absence of these conservative processes, would be capable of causing reaction to be continued for a very long time. We have here, then, not a mere coming to the state of mechanical equilibrium with reference to the environment—a result which ultimately comes to all biological systems in death—but the achievement of this end with the bodily energy reserves still unexhausted and the mechanism for motion intact. To effect this, some kind of active, compensatory process is necessary, parallel with the systemic displacements of a chemical system obeying the Le Chatelier law.

(2a, 25, 12) without even the intervention of a nervous system, while at the other end of the scale it has been observed in differentiated nervous tissue, without receptor or synapsis (1).

The equilibrium involved in negative adaptation is static; the organism starts with a state of rest, is disturbed, and again, by some specific checking process, achieves rest. But the case is not always so simple. We have seen that the biological system is so organized that depletion of energy reserves initiates periodic displacements which cease only when food is reached. Suppose that on a series of such occasions the conditions of the food search are all different. Then the details of each food-excursion will be different, the only thing in common to such successive searches being the fact that food is reached at the end. The sea gull may fly after a ship on one search for food, may swim on the water on another, may settle in a field on a third; on all three occasions it may ultimately obtain nourishment. But suppose the food-excursions occur on a number of successive occasions under identical conditions. What actually happens, in the case of the higher organisms, is that on the first such occasion, relatively much space is traversed, much time spent, and much energy used up; but as the performance is repeated there is a gradual diminution of these expenditures until they are cut down to a minimum, where they stay as long as the feat is 'practised,' that is, as long as the organism is confronted with the same conditions when it is 'hungry.' That is to say, a 'steady state' has been reached, an equilibrium which is not static but dynamic. The achievement of such a steady state is usually called learning. When the word learning is used, it is implied that there is a connection of some kind between the successive performances. They converge to the same learned performance, later terms being impossible without earlier ones. They form, in fact, part responses in the whole Response to the four-dimensional Situation, which is, let us say, the presentation-of-maze-with-food-at-4-hour-intervals. If again it seems artificial thus to group 'separate' situations in a whole situation, it may then be pointed out that the very nature of

the 'learning' idea implies that these 'situations' are not separate. Since they must be grouped together as a whole, clearly the proper way of describing this whole is in the terms provided by present-day physical science—which in any case we have no excuse for neglecting in favour of the Mid-Victorian system.

But not every four-dimensional environmental manifold may thus condition a constant state on the part of the organism. For this there must ordinarily be something constant on the environmental side of the relation. It is difficult, perhaps impossible to conceive of learning as giving improved performance in a totally different set of conditions. In the usual terminology, some kind of repetition is ordinarily necessary, whether this may involve identical 'situations' or elements, Gestalten, ratios, or what not.

If, for example, a man has 'learned' where to find mushrooms in the park, he has in the past looked in the same park or in other similar situations. When this piece of learning is complete, the time and energy spent on the task is minimal, by comparison with that spent on previous occasions. His physical system has in fact come to a position of equilibrium with reference to this whole situation; it has made an adjustment to the total four-dimensional manifold, the situation differing from that presenting itself on any specific search in that the greater one includes the less. The relation is the same as that between a pattern formed of circles and a single circle, except that the total manifold to which the man reacts is four-dimensional. And just as the organism may make a single unified adjustment, reaction, to such a manifold of circles, so we may regard it as making a single, unified adjustment to such a manifold of park-situations. The 'repetitive' aspect is still clearer in the case of maze learning, where the utmost care is taken to maintain 'standard' conditions.

Learning thus appears as an extension of the organism's power of adjustment to environmental changes, and may indeed properly be called the act of adjustment to a four-dimensional situation such as has been described. It appears to be unique because we are in the habit of neglecting the

time-coördinate in regarding biological phenomena, and thus naturally consider a tridimensional stimulus-complex as a single whole-situation to which the organism may react, while to consider a four-dimensional stimulus-complex as such a whole-situation seems mildly unnatural. But the intellectual transition must be made if our descriptions are to fit the facts. "Murder," says Whitehead, "would be the prerequisite for the absorption of biology into physics, as expressed in the traditional concepts." The older concepts are at least inadequate to a real description of organic as they have been found to be of inorganic phenomena.

This conception of learning seems to throw light on certain other problems of psychology. When an animal has 'learned' a maze, three things have happened. (*a*) There has been established between the four-dimensional organic system and the environment a dynamic equilibrium. (*b*) This involves, on the part of the organism, the synthesis of a four-dimensional neural pattern which is not necessarily anatomically fixed, and will involve elements corresponding to both the nutritional and the non-nutritional factors of the environment. The evidence for such a unified neural pattern is fast accumulating. Lashley, for example, has lately stated that "none of the studies of learning or retention of the mazes after cerebral lesions has given the slightest indication that the maze habit is made up of independent associational elements" (**13a**, p. 141), and has shown elsewhere (**13**) that once action has been initiated, the appropriate sequence of response will follow without even the assistance of sensory cues.[6] (*c*) The neural pattern is such that reaction to the non-nutritional factors of the environment has by comparison with previous performances a minimal form. That is, the space-time line representing such performance is ultimately a minimum. These three facts may be called the principles of equilibrium,

[6] The 'synthesis' may take place suddenly, perhaps by a 'physiological short circuit' (Wertheimer) or by some other process. *Cf.* Lashley (13a), who refers on this point to the evidence of the form of the learning curves. Such sudden synthesis seems to be the physiological basis of Professor Köhler's insight. Köhler has performed a fundamental service in pointing out that such sudden adjustment does in many cases take place.

association (or synthesis), and improvement of performance towards a minimum. They are three aspects of the total occurrence, and have been separately studied in three sets of apparently disconnected experiments, which have each brought one of them into relief. I refer, of course, to the experiments on negative adaptation, conditioned reflexes, and maze running, each of which stresses a single phase of the total four-dimensional process. The conditioned reflex is thus to be regarded as a maze experiment with the length of the run cut down to zero, or conversely, the maze experiment as a conditioned reflex with a highly expanded secondary stimulus, giving opportunity for the natural locomotion to take place.

Finally, as to 'elimination of errors.' An 'error' is a response that can be eliminated by an animal in search of food. Therefore, in general, if the nervous system of the animal is such that it can take up an equilibrium to successive presentations of the maze—and not all nervous systems are such—the error will be eliminated. Elimination of action goes on until by the conditions of the experiment no more elimination is possible. "Often" says Mach (**18**), when speaking of the establishment of equilibrium in organic complexes, "the phenomena of nature exhibit maximal or minimal properties because when these . . . are established the causes of all further alteration are removed. . . . It sounds much less imposing but is much more elucidatory, much more correct and comprehensive, instead of speaking of the economical tendencies of nature, to say, 'So much and so much only occurs as in virtue of the forces and circumstances involved can occur.'" Equilibrium implies a minimal path, and a minimal path implies elimination of 'errors.' As to the nature of the process by which the nervous system achieves this result little can at present be said with certainty. The attempt has been made to give a restatement, not a solution, of the problem.

Thus we are enabled to obtain an evolutionary picture of learning. In the primal clash, inorganic complexes of all kinds would be formed. Some of them would have survival value, in that they were so organized that they tended to

persist in the face of changing conditions. Thus the elements were formed, and the stable compounds. When a complex had such survival value, it must have possessed the organization necessary to initiate of itself compensatory changes when external conditions altered. Later appeared the simpler forms of life, more elaborate complexes, which survived or not according as they had the requisite organization to maintain their integrity.[7] Change in the external vital conditions of such an organic complex is a 'stimulus'; the resulting systemic changes, adjustments, made by the organism, are 'responses'; a complex of stimuli is called a situation. As biological evolution progressed, organic complexes were able to respond more and more intimately to changes in the environment, until there was developed the power to make a dynamic adjustment to a highly complex four-dimensional manifold. This is the power to Learn.

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[7] The organism is of course not a closed or isolated system; by interchange of matter and energy with the environment it is chemically *stationary*, not stable. (W. Ostwald, Publications in Physiology, Univ. Cal., 1903, 1, pp. 11–31.)

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KNOWLEDGE AND PURPOSE AS HABIT MECHANISMS

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It is only with the greatest difficulty that scientists are able to maintain a thoroughly naturalistic attitude toward the more complex forms of human behavior. Our intellectual atmosphere is still permeated in a thousand subtle ways with the belief in disembodied behavior functions or spirits. The situation is aggravated by the fact that the details of the more complex action patterns are so concealed as to be almost impossible of observation. Even so, the outlook is hopeful. The work of many ingenious investigators is bringing to light important details of the hidden processes, and enough evidence has already accumulated to enable us in a number of cases to discern with tolerable clearness the broad naturalistic outlines of their operation.

I

One of the oldest problems with which thoughtful persons have occupied themselves concerns the nature and origin of knowledge. How can one physical object become acquainted with the ways of another physical object and of the world in general? In approaching this problem from the point of view of habit, it is important to recognize that knowledge is mediated by several fairly distinct habit mechanisms. In the present study but one of these will be elaborated.

Let us assume a relatively isolated inorganic world sequence taking place as shown in Fig. 1. Here S_1, S_2, etc.,

THE WORLD: $S_1 \longrightarrow S_2 \longrightarrow S_3 \longrightarrow S_4 \longrightarrow S_5 \longrightarrow$ --

FIG. 1

represent typical phases of a sequential flux, the time intervals between successive S's being uniform and no more than a few

seconds each. Let us suppose, further, that in the neighborhood of this world sequence is a sensitive redintegrative organism. The latter is provided with distance receptors and is so conditioned at the outset as to respond characteristically to the several phases of the world sequence. Each *S* accordingly becomes a stimulus complex impinging simultaneously on numerous end organs. As a result, each phase of the world sequence now becomes a cause, not only of the succeeding phase in its own proper series, but also of a functionally parallel event (reaction) within the neighboring organism. The organismic responses of the series thus formed have no direct causal relationship among themselves.[1] R_1 in itself has no power of causing (evoking) R_2. The causal relationship essential in the placing of R_2 after R_1 is that of the physical world obtaining in the *S*-sequence; R_2 follows R_1 because S_2 follows S_1. The situation is represented diagrammatically in Fig. 2.

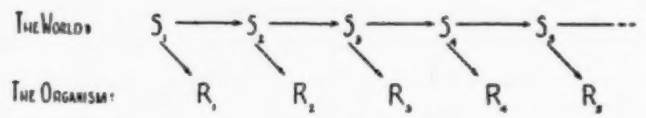

Fig. 2

Now a high-grade organism possesses internal receptors which are stimulated by its own movements. Accordingly each response (*R*) produces at once a characteristic stimulus complex and stimuli thus originated make up to a large extent the internal component of the organism's stimuli complexes. Let these internal stimulus components be represented by *s*'s. If we assume, in the interest of simplicity of exposition, that the time intervals between the phases of the world flux selected for representation are exactly equal to those consumed by the $S \rightarrow R \rightarrow s$ sequences, the situation

[1] This neglects the original dynamic influence of the ever-present internal component of the organismic stimulus complex into which each phase of the world sequence enters to evoke the corresponding organismic reaction. The excitatory potency of this internal component is here supposed to be minimal. Its influence is neglected in the interest of simplicity of exposition. Its undeniable presence clearly introduces an element of subjectivity into reactions which appear superficially to be evoked purely by the external world.

will be as shown in Fig. 3, S_2 coinciding in time with s_1, S_3 with s_2 and so on.

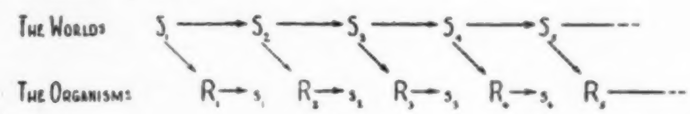

FIG. 3

Now, by the principle of redintegration, all the components of a stimulus complex impinging upon the sensorium at or near the time that a response is evoked, tend themselves independently to acquire the capacity to evoke substantially the same response. We will let a dotted rectangle indicate that what is enclosed within it constitutes a redintegrative stimulus complex; and a dotted arrow, a newly acquired excitatory tendency. After one or more repetitions of the world sequence, the situation will be as shown in Fig. 4.

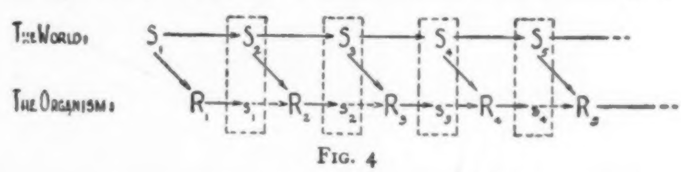

FIG. 4

As a result of the joint operation of the several factors summarized in Fig. 4, the organismic reactions (*R*'s) which at the outset were joined only by virtue of the energies operating in the outer world sequence of *S*'s, are now possessed of a genuine dynamic relationship lying within the organism itself. To make this clear, let it be assumed that the world sequence begins in the presence of the organism, but is at once interrupted. The resulting situation is shown diagrammatically in Fig. 5. The newly acquired excitatory tend-

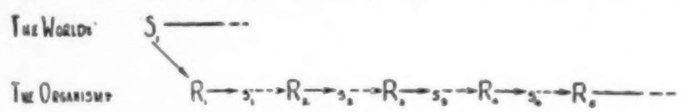

FIG. 5

encies, unless interrupted by some more potent influence, should continue the organismic sequence of responses very much as when they were first called forth as the result of the stimulation by the world sequence.

In summary it may be said that through the operation of a variety of principles and circumstances, the world in a very important sense has stamped the pattern of its action upon a physical object. The imprint has been made in such a way that a functional parallel of this action segment of the physical world has become a part of the organism. Henceforth the organism will carry about continuously a kind of replica of this world segment. In this very intimate and biologically significant sense the organism may be said to know the world. No spiritual or supernatural forces need be assumed to understand the acquisition of this knowledge. The process is entirely a naturalistic one throughout.

II

Once the organism has acquired within its body this subjective parallel to the ways of the physical world, certain other activity patterns or habit mechanisms at once become operative. One of the more important of these is the power of foresight or fore-knowledge. A great deal of mystery has surrounded this problem. Foresight may be defined for our present purpose as *the reaction to an event which may be impending, but which has not as yet taken place.* The difficulty seems largely to have been concerned with the problem of how an organism can react to an event not yet in existence. The reasoning runs: An event not yet in existence cannot be a stimulus; and how can an organism react to a stimulus which does not exist? In terms of our diagram, how can R_5, which is a reaction to the stimulating event S_5, take place before S_5 itself has occurred?

An important circumstance connected with foresight is the fact that the tempo of the acquired subjective parallel to the outer world sequence is not limited to that of the latter. Indeed, there is evidence indicating a tendency for a primary conditioned reaction to run off at a higher speed than that of the master world sequence which it parallels.[2] Thus it comes

[2] C. L. Hull, A functional interpretation of the conditioned reflex, Psychol. Rev., 1929, 36, p. 507 *ff*. A quite distinct mechanism serving much the same function as that here emphasized has its basis in the peculiar advantage afforded by distance

about that, even when both series begin at the same instant, the end-reaction of the subjective series may actually antedate the stimulus in the world sequence which exclusively evoked it previous to the conditioning shown in Fig. 4. It is evident that this possibility of the heightened tempo on the part of the organismic act sequence is intimately connected with the possession by the organism of knowledge of events before they actually take place.

The biological advantage of antecedent knowledge of impending events is great. This is particularly clear in the case of defense reactions. These latter fall into two main types—flight and attack. Let us suppose that in the example elaborated above, S_5 is a seriously nocuous stimulus and R_5 is a successful flight reaction. Foresight will result from the reeling off of the R-series faster than the S-series so that s_4 will evoke R_5 before S_5 has occurred. In this event S_5, when it does occur, will not impinge on the organism for the reason that the latter will have withdrawn from the zone of danger as the result of the act R_5. In case R_5 is an act of attack rather than flight it must, to be successful, bring the organismic series into contact with the world sequence in such a manner as to interrupt the latter before S_5 is reached. In this case also, the organism clearly escapes the injury. Thus the supposed impossibility of an organismic reaction to a situation before it exists as a stimulus is accomplished quite naturally through the medium of an internal substitute stimulus.

III

A reflective consideration of the habit mechanisms involved in anticipatory defense reactions reveals a phenomenon of the greatest significance. This is the existence of acts whose sole function is to serve as stimuli for other acts. We shall accordingly call them *pure stimulus acts*. Under normal conditions practically all acts become stimuli, but ordinarily

receptors. The stimulus of a distant object through a distance receptor is often sufficiently like that when the object is near and nocuous to evoke a successful defense reaction before the source of danger can get near enough to produce injury. This has been discussed in detail by Howard C. Warren, *J. Phil., Psychol. & Scient. Meth.* 1916, 13, p. 35 *ff*.

the stimulus function is an incidental one. The consideration of the approach of an organism to food may clarify the concept. Each step taken in approaching the food serves in part as the stimulus for the next step, but its main function is to bring the body nearer the food. Such acts are, therefore, primarily instrumental. By way of contrast may be considered the anticipatory defense sequence presented above. R_5, the actual defense reaction, obviously has instrumental value in high degree. R_4, on the other hand, has no instrumental value. This does not mean that it has no significance. Without R_4 there would be no s_4, and without s_4 there would be no R_5 *i.e.* no defense. In short, R_4 is a pure stimulus act. In the same way R_3 and R_2 serve no instrumental function but, nevertheless, are indispensible as stimulus acts in bringing about the successful defense response.

A simple experiment which can be performed by anyone in a few moments may still further clarify the concept of the pure stimulus act. Ask almost any psychologically naïve person how he buttons his coat with one hand—which finger, if any, he puts through the buttonhole, what the last act of the sequence is—and so on. The average person can tell little about it at first. If wearing a coat, he will usually perform the act forthwith. If warned against this, the hand may quite generally be observed to steal close to the position at which the buttoning is usually performed and to go through the buttoning behavior sequence *by itself*. After this the nature of the final buttoning act may be stated with some assurance. Clearly, the earlier acts of this pseudo-buttoning sequence are pure stimulus acts since they serve no function whatever, except as stimuli to evoke succeeding movements and ultimately, the critical final movement which is sought.

It is evident upon a little reflection that the advent of the pure stimulus act into biological economy marks a great advance. It makes available at once a new and enlarged range of behavior possibilities. The organism is no longer a passive reactor to stimuli from without, but becomes relatively free and dynamic. There is a transcendence of the limitations of habit as ordinarily understood, in that the organism can

react to the not-here as well as the not-now. In the terminology of the *Gestalt* psychologists, the appearance of the pure stimulus act among habit phenomena marks a great increase in the organism's 'degrees of freedom.' The pure stimulus act thus emerges as an organic, physiological—strictly internal and individual—symbolism.[3] Quite commonplace instrumental acts, by a natural reduction process, appear transformed into a kind of *thought*—rudimentary it is true, but of the most profound biological significance.

Thus the transformation of mere action into thought, which has seemed to some as conceivable only through a kind of miracle, appears to be a wholly naturalistic process and one of no great subtlety. Indeed, its obviousness is such as to challenge the attempt at synthetic verification from inorganic materials. It is altogether probable that a 'psychic' machine, with ample provision in its design for the evolution of pure stimulus acts, could attain a degree of freedom, spontaneity, and power to dominate its environment, inconceivable alike to individuals unfamiliar with the possibilities of automatic mechanisms and to the professional designers of the ordinary rigid-type machines.

IV

Pure stimulus-act sequences present certain unique opportunities for biological economy not possessed by ordinary instrumental-act sequences. In the first place, there is the ever present need of reducing the energy expenditure to a minimum while accomplishing the ordinary biological functions in a normal manner. It is clear that pure stimulus-act sequences, since they no longer have any instrumental function, may be reduced in magnitude to almost any degree consistent with the delivery of a stimulus adequate to evoke

[3] This peculiarly individual form of symbolism is not to be confused with the purely stimulus acts of social communication. Neither is it to be confused with what appears to be a derivative of the latter by a reduction process, the subvocal speech emphasized by Watson. The special stimulus-response mechanisms by which the evolution of these latter forms of symbolism take place, together with their peculiar potentialities for mediating biological adjustment and survival, are so complex as to preclude consideration here.

the final instrumental or goal act.[4] Observation seems to indicate that this economy is operative on a very wide scale. It may even be observed in the buttoning experiment previously cited. The hand while going through the buttoning sequence by itself will ordinarily make movements of much smaller amplitude than when performing the instrumental act sequence with a real button.

A significant observation made by Thorndike in the early days of animal experimentation illustrates the same tendency, though in a very different setting. He placed cats in a confining box from which they sought to escape. Some he would release only when they licked themselves, others only when they scratched themselves. After an unusually long training period the cats finally learned to perform the required acts and thus to escape fairly promptly. In this connection, Thorndike remarks:

> "There is in all these cases a noticeable tendency, of the cause of which I am ignorant, to diminish the act until it becomes a mere vestige of a lick or a scratch. After the cat gets so it performs the act soon after it is put in, it begins to do it less and less vigorously. The licking degenerates into a mere quick turn of the head with one or two motions up and down with tongue extended. Instead of a hearty scratch the cat waves its paw up and down rapidly for an instant." [5]

The ordinary scratch of a cat is an instrumental act. It must have a certain duration and intensity to serve its function. In the present instance the scratch served only as a visual stimulus to Dr. Thorndike. As such, a small movement was presumably quite as effective as a large one.

In the second place there is, particularly in the case of primitive defense acts, the need to economize time so as to increase the promptness of the defense reaction. This de-

[4] Movements greatly reduced in magnitude tend to become vestigial. This suggests a possible explanation of the extreme subjectivity of imagery. Just how far the weakening of pure stimulus acts may go and still serve their stimulus function is a question which may yield to experimental approach. That they should diminish to an actual zero, with nothing but a neural vestige remaining to perform the stimulus function, is conceivable though hardly probable. It is believed that the present hypothesis is general enough to fit either alternative.

[5] E. L. Thorndike, Animal intelligence, MacMillan, 1911, p. 48.

sideratum appears to be accomplished by the same means as the first—the reduction in the magnitude of the acts. A movement of small amplitude should be more quickly performed than one of large amplitude.

But the maximum of economy, both as to energy and as to time, demands not only that the units of the stimulus-act sequence shall be small in amplitude, but that they shall also be as *few* as possible. If a single stimulus-act is sufficient to furnish the necessary stimulus for the defense reaction, the existence of all the other stimulus acts in the series is a sheer waste, both of time and energy. This means that biological efficiency demand on two separate counts the dropping out of large sections of purely stimulus-act sequences.

V

The importance of the serial-segment elimination tendency in pure stimulus-act and other complex learning sequences raises very insistently the question as to what stimulus-response mechanisms may bring it about. Observation suggests that one condition favorable for 'short circuiting' is that the process shall be strongly 'purposive.' *In the present study the purpose mechanism shall be understood as a persisting core of sameness in the stimulus complexes throughout the successive phases of the reaction sequence.* We will symbolize this persisting stimulus by S_p. This may be thought of concretely as a continuous strong red light, or a continuous gripping of a dynamometer, or the continuous knitting of the brows, or (more typically) the continuously recurring crampings of the digestive tract as in hunger.

When the principle of the persisting stimulus is joined to the set of principles represented as operating in Fig. 5, a number of novel consequences at once appear. The situation is represented in Fig. 6. An examination of this diagram shows that S_p has a unique advantage over all the other components in the several stimulus complexes. Thus, S_1, S_2, etc. and s_1, s_2, etc. can get conditioned, except for remote associative tendencies,[6] only to the response in each case

[6] These are here neglected in order to simplify the exposition. Ultimately they must, of course, be taken fully into account.

which immediately follows, *i.e.* to but a single response each. But S_p, since it is present in all the stimulus complexes of the series, *gets conditioned to all the reactions* taking place in it.

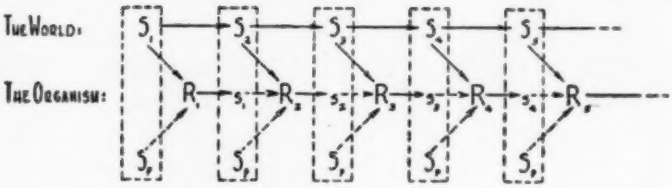

Fig. 6

This multiplicity of excitatory tendencies resulting from the situation shown in Fig. 6, is represented diagrammatically in Fig. 7.

Fig. 7

VI

It is evident that in a situation such as is presented in Fig. 7, a competition of the several excitatory tendencies will follow. Since this competition must be between the several parts of the series, it will be called *intraserial competition.* We may safely assume that the several excitatory tendencies radiating from S_p will have varying strengths. There also enter into this competition, of course, the stimulus elements which may be present in the stimulus complex from other sources at any particular moment. We will simplify the stimulus situation somewhat by assuming that the world sequence is interrupted at once after its first phase, S_1. What, then, will be the state of this intraserial competition at the second stimulus complex of the diagram?

If we assume that s_1 has an excitatory tendency toward R_2 of 2 units, that S_p also has an excitatory tendency toward

R_2 of 2 units, toward R_3 of 3 units, towards R_4 of 4 units and towards R_5 of 5 units, the competition among the several segments of the series will be that shown in Fig. 8. From

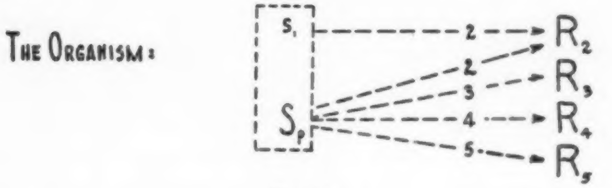

FIG. 8

this diagram it may be seen that the immediately following reaction (R_2) in the original action sequence has the advantage of a double excitatory tendency, whereas the more distant reactions such as R_3, R_4, and R_5, have but a single excitatory tendency each, that arising only from S_p. But if at any time one of the single (S_p) excitatory tendencies should chance to be stronger than the combination of the two tendencies leading to the immediately following act of the original sequence, the elimination of a segment of the pure stimulus-act sequence will take place.

In order to understand how the purposive mechanism, through intraserial competition, may bring about serial segment elimination, let us observe the sequel to the following hypothetical situation. It may very well prove to be the case that S_p gets conditioned more strongly as the final or critical response in a behavior cycle is approached.[7] Accordingly a rough approximation to such a system of excitatory tendencies has been assigned to the bonds presented in Fig. 8. We may summarize the several competing excitatory tendencies radiating from the second stimulus complex as follows:

[7] It would not appear to be an over difficult task to test this hypothesis experimentally. If it should prove true it would have extensive theoretical implications and would clear up a number of questions in the theory of learning. However, almost any other hypothesis which provides considerable variation in the strength of the excitatory tendencies radiating from S_p will produce substantially similar results. It may be added that an irregular distribution of intensities of excitatory tendencies from S_p offers special opportunities for backward serial segment elimination as contrasted with the more usual forward variety here emphasized.

$$R_2 = 4,$$
$$R_3 = 3,$$
$$R_4 = 4,$$
$$R_5 = 5.$$

This shows that the reaction following the second stimulus complex must be, not R_2 as in the original act sequence, but R_5. But if R_5 follows immediately after R_1, the behavior segment shown in Fig. 9 drops completely out of the series. This is inevitable because no stimulus now remains in the series adequate to evoke it.

THE ORGANISM: $R_2 \longrightarrow s_2 \dashrightarrow R_3 \longrightarrow s_3 \dashrightarrow R_4 \longrightarrow s_4$

FIG. 9

One of the most baffling theoretical problems related to experimental psychology has been that of explaining how errors or unnecessary acts in behavior sequences get eliminated. Nevertheless, few psychological phenomena are more common. One is asked the product of 49 × 67. He writes the numbers down on paper, certain multiplication-table and addition-table habits of childhood are evoked in an orderly succession, and at length there is written down by successive stages the number, 3283. If, not too long afterwards, the individual is again asked the product of 49 × 67, he may respond by saying 3283 at once. In thus passing directly from the question to the answer, the behavior sequence of pure stimulus acts which constituted the detailed multiplication of 49 × 67 has completely dropped out of the sequence.

The difficulty of accounting for this phenomenon has been due to a considerable extent to the fact that the serial segment elimination must take place in the face of the so-called law of use or frequency. According to this principle (alone) practice or repetition might be expected blindly to fix the undesirable behavior segment in its place more firmly than ever. Perhaps such inadequacies as these have contributed much to bringing the simple chain reaction theory into its deserved ill repute as a universal explanatory principle. As a matter of plain fact,

the principle of redintegration from which may be derived the simple chaining of reactions, implies with equal cogency the evolution of a stimulus-response mechanism which appears to be capable on occasion of completely transcending the chaining tendency.[8] According to this principle any stimulus such as an organic craving which persists as a relatively constant component throughout the otherwise largely changing stimulus complexes of a behavior sequence, must become conditioned to every act of the series. The implications of this for complex adaptive behavior are far reaching. It is our present concern only to point out that the persisting stimulus, through the sheaf of excitatory tendencies emanating from it to every act of the series, provides a unique dynamic relationship between each part of the series and every other part. This, as we have seen above, gives rise to a significant competition among the several potential action tendencies within the series. While final decision must be reserved until the facts are determined by experiment, the probability seems to be that this intraserial competition may easily become sufficiently potent to over-ride the simple chain-reaction tendency and produce a leap in the behavior sequence from the beginning of a series at once to the final or goal reaction, thus eliminating the intervening unnecessary action segment.

VII

The results of the present inquiry may be briefly summarized.

Sequences in the outer world evoke parallel reaction sequences in sensitive organisms. By the principle of redintegration the organismic sequences acquire a tendency to run off by themselves, independently of the original world sequences. The organism has thus acquired an intimate functional copy of the world sequence, which is a kind of knowledge.

In case the two sequences begin at the same time but the organismic or behavior sequence runs off at a faster rate, the knowledge becomes fore-knowledge or foresight. This has great significance in terms of biological survival.

[8] See E. L. Thorndike, The original nature of man, New York, 1913, 186–187.

The possibility of more or less extended functional habit sequences being executed by the organism with an instrumental act only at the end, gives rise to the concept of the pure stimulus act. Such behavior sequences have great biological survival significance because they enable the organism to react both to the not-here and the not-now. Incidentally it accounts for a great deal of the spontaneity manifested by organisms.

The concept of the pure stimulus act appears to be the organic basis of symbolism but is believed to be a more fundamental one than that of symbolism as ordinarily conceived.

Pure stimulus-act sequences offer possibilities of biological economy, both of energy and of speed, through the reduction in the amplitude of the acts in the sequence. Further analysis reveals the fact that both energy and time would be economized with no incidental sacrifice if the acts between the beginning of an action cycle and its goal act should drop out of the sequence. Observation seems to show that the dropping out of such intervening pure stimulus acts occurs very extensively.

The problem arises as to how this dropping out of undesirable behavior segments may come about, since it appears to be a violation of the 'law of use.' A plausible explanation is found in the peculiar potentialities of stimuli which persist relatively unchanged throughout a behavior sequence. A persisting stimulus component is regarded as one of the characteristic mechanisms of purposive behavior. We should expect such a stimulus to get conditioned to every act of the sequence, presumably most strongly to the goal act and those acts immediately preceding the goal act. The resulting multiplicity of excitatory tendencies emanating from the persisting stimulus is found to generate an important phenomenon—the competition among the several potential segments of the behavior series. This intraserial competition, if sufficiently strong, could easily over-ride the simple chaining of contiguous acts produced by the 'law of use' and enable the final act of the original series to be evoked at once after the first act of the series, thus producing what is rather inappropriately

called 'short-circuiting.' Thus may a persistent problem in the theory of mammalian adaptive behavior be on its way to solution.

The general plausibility of the foregoing theoretical deductions as well as the probable biological significance of several of the deduced mechanisms, suggests strongly the desirability of an intensive program of experimental research designed to test their actuality. In that way the true function of theoretical analysis may be realized.

[MS. received June 5, 1930]

THE PUBLICATIONS OF AMERICAN PSYCHOLOGISTS

BY SAMUEL W. FERNBERGER

University of Pennsylvania

Some time ago we printed a statistical analysis of the data contributed by Members and Associates of the American Psychological Association to the Year-Book of that Association.[1] In this paper were analysed such factors as geographical distribution, academic antecedents, the problem of inbreeding, subjects of instruction and finally research interests.

The present study is a much more detailed analysis of this last factor of research interest. The former analysis indicated a very strong emphasis in applied aspects of psychology rather than in psychology as a scientific discipline. The criticism may be made that in this former study we have analysed only the Year-Book data or, in other words, what the psychologists have *said* about themselves. The present study attempts to rectify this possible source of error by studying what these individuals have actually *done* and published of a research nature. An effort has been made to keep this study as objective as possible.

Due to the magnitude of the task, we have set up certain arbitrary limits to the investigation. The group here considered were those individuals listed as Members only in the 1928 Year-Book of the Association. These comprise the senior group of American psychologists and the justification for the elimination of the Associates from this study is that they are, on the whole, a junior group of more recent date and therefore with less chance to produce research contributions. The research publications considered were limited to those listed in the PSYCHOLOGICAL INDEX for the years

[1] S. W. Fernberger, Statistical analyses of the members and associates of the American Psychological Association in 1928, PSYCHOL. REV., 1928, 35, 447–465.

1919–1928, both dates inclusive. This ten-year period covers the time of greatest productivity of American psychologists and is also the period of the most inclusive editing of the INDEX.

The method employed was simple. The Year-Book data were posted on a separate card for each Member and his references were looked up in each of the ten numbers of the INDEX. Each reference was listed on the card in accordance with the INDEX listing as to the field of research. In some cases the INDEX listing may not be correct, but it is our opinion that these errors are so few in number as not to effect the statistical validity. Some idea of the magnitude of the task can be given when it is noted that we considered 421 men and 121 women, for whom we found a total of 3,862 titles. Of this total we eliminated 94 titles by 60 individuals which appeared in popular magazines and which could not be considered as research contributions,—leaving a total of 3,768 titles considered in the present study.

TABLE I

DISTRIBUTION OF PUBLICATION BY YEARS

Year	Men	Women	Total
1919	227	44	271
1920	297	56	353
1921	269	22	291
1922	327	54	381
1923	333	49	382
1924	305	38	343
1925	334	62	396
1926	399	63	462
1927	381	57	438
1928	401	50	451
Totals	3,273	495	3,768

The distribution of these titles by years will be found in Table I. One observes an increase in the number of titles for men for successive years but no appreciable increase in the number of titles for women. In the case of neither sex is the increase of titles comparable with the increase in membership over the same period of time.

A significant sex difference already appears in the data of

this table. The average number of contributions over the ten-year period are 7.77 for men and only 4.09 for women. The men Members of the Association have been almost twice as productive, on the average, as the women.

This sex difference comes out clearly in the question of the number of publications produced by different individuals, as presented in Table II. It will be seen that there are

TABLE II

CONSISTENCY OF PUBLICATION

Number of Publications	Men	Women	Total
0	68	25	93
1	34	21	55
2	47	9	56
3	34	20	54
4	20	12	32
5	25	7	32
6	21	7	28
7	17	5	22
8	17	4	21
9	14	2	16
10 or over	124	9	133

approximately $3\frac{1}{2}$ times as many men as women Members of the Association. The results in Table II indicate that only $2\frac{1}{2}$ times as many men failed to produce anything in the 10-year period; while almost 14 times as many men as women produced 10 articles or more over the same period.

The results with regard to the different fields are given in Table III. A comparison of sex differences is of interest. Experimental Psychology is largely in the hands of the men; Clinical Psychology less so, with women contributing their appropriate share. Probably the greatest differences are to be found in the group of purely Theoretical papers and in the fields of Social and Animal Psychology and Statistics, but very marked differences are to be found in all of the fields with the exception of Child Psychology. Here the output has been relatively small, but it is the one field to which women have contributed more than their appropriate share of research.

One fact is of interest which does not show in the statistical

TABLE III

ANALYSIS OF PUBLICATIONS IN DIFFERENT FIELDS

	Men		Women		Total	
	Number of Articles	Number of Individuals	Number of Articles	Number of Individuals	Number of Articles	Number of Individuals
Experimental	675	157	120	26	795	183
Clinical	597	149	155	56	752	205
Theoretical	555	154	40	11	595	165
Educational	337	102	53	31	390	133
Abnormal	242	70	32	17	274	87
Applied	207	59	33	16	240	75
Social	216	85	13	9	229	94
Historical	191	82	15	12	206	97
Animal	148	45	6	6	154	51
Statistics	73	30	5	5	78	35
Child	32	24	23	16	55	40
Popular	89	55	5	5	94	60
Total (ex popular)	3273		495		3768	

analysis. Of the 595 titles listed under Theoretical, *104 are systems of psychology* written in text book or other form. These 104 systems of psychology were written by 70 individuals which means that, in many cases, the same individual produced two or even, in a few cases, three systems of psychology within the ten-year period.

TABLE IV

NUMBER OF FIELDS IN WHICH PUBLICATION HAS APPEARED

Fields	Men	Women	Total
1	104	32	136
2	98	34	132
3	82	18	100
4	51	8	59
5	33	1	34
6	22	2	24
7	11	1	12
8	4	—	4
Total	405	96	501

Another interesting sex difference appears in Table IV, in which are listed the number of fields in which different individuals have worked. The table indicates that men have much broader research interests as a group. While only 12 women or 13.5 per cent have worked in 4 or more fields, the number of men is 121 or 29.9 per cent.

There can be no question with regard to the existence of these sex differences. Undoubtedly they have a complex *economic* basis which it is not possible to describe from the data at hand. These sex differences in productivity remain constant, however, throughout all of the further analysis. For the sake of brevity, then, the results for men and women will be combined in the future discussion.

GEOGRAPHICAL DISTRIBUTION

The effects of geographical distribution on productivity will be found in Tables V–VII. The nine geographical areas are chosen in accordance with the grouping of states by the U. S. Census Bureau. In Table V are the results of the number of publications for each geographical area. Every

area contains at least one individual who has produced an average of at least one manuscript each year. Also in each area but one there were individuals, who did not publish anything. But the results are plain that the North and Central Atlantic Seaboard and the Northeast Central areas are the great producing regions in psychology with the South Atlantic and Pacific areas as poor seconds. And the Southwest Central and Mountain regions are the Great American Desert—speaking in terms of psychological research.

TABLE V

NUMBER OF PUBLICATIONS AND GEOGRAPHICAL DISTRIBUTION

No. of Publications	0	1	2	3	4	5	6	7	8	9	10 or over
New England	12	6	8	4	4	2	6	2	3	3	23
Middle Atlantic	25	17	21	14	8	11	7	8	7	8	34
N. E. Central	19	9	7	9	13	11	7	5	2	1	32
N. W. Central	7	8	7	9	2	2	3	2	2	2	9
South Atlantic	10	4	3	7	—	3	—	3	3	1	11
S. E. Central	5	—	2	3	1	—	1	—	—	1	1
S. W. Central	2	3	—	1	—	—	1	—	1	—	1
Mountain	—	4	2	—	2	—	—	2	—	—	2
Pacific	10	3	7	5	2	2	1	1	3	—	13
Outside U. S. A.	3	1	—	1	—	1	2	—	—	—	4

Exactly similar results for consistency of publication and geographical area are found in Table VI. Those areas which

TABLE VI

CONSISTENCY OF PUBLICATION AND GEOGRAPHICAL DISTRIBUTION

	1	2	3	4	5	6	7	8	9	10
New England	6	10	9	5	7	8	4	6	5	1
Middle Atlantic	22	25	23	18	12	16	6	7	4	4
N. E. Central	11	14	16	18	7	4	9	6	8	3
N. W. Central	13	8	7	8	2	4	1	1	1	1
South Atlantic	5	4	6	5	3	3	1	4	2	2
S. E. Central	1	1	3	2	—	—	1	—	—	1
S. W. Central	3	—	1	1	1	1	1	—	—	—
Mountain	4	2	1	2	1	—	—	1	—	1
Pacific	3	11	5	3	3	2	2	6	2	—
Outside U. S. A.	1	1	—	2	2	—	—	2	1	—

have produced the largest number of titles, as shown in Table V, also show the greatest consistency of publication.

TABLE VII

FIELD AND GEOGRAPHICAL DISTRIBUTION

	Experimental	Clinical	Theoretical	Educational	Abnormal	Applied	Social	Historical	Animal	Statistics	Child
New England	118	86	135	45	69	18	17	54	28	4	4
Middle Atlantic	167	226	169	132	63	123	66	65	33	22	13
N. E. Central	167	170	136	119	26	66	50	46	21	29	10
N. W. Central	65	67	40	38	11	2	19	15	7	2	14
South Atlantic	147	45	43	21	37	11	36	5	19	2	4
S. E. Central	8	21	10	7	3	1	2	2	1	1	—
S. W. Central	6	7	4	3	5	1	—	4	1	—	1
Mountain	22	27	8	7	4	—	8	5	3	—	—
Pacific	69	68	32	33	29	19	9	4	28	12	3
Outside U. S. A.	15	25	11	2	9	—	8	—	6	—	6

The author believes that consistency of publication is as good an index of research interest as the number of titles. For any individual to produce in each of 7 or 8 years of a ten-year period indicates a really sustained research interest in which publication is not merely an occasional affair.

Table VII contains the results with regard to geographical distribution of the authors and the field in which the contributions appear. New England, the Middle and South Atlantic and Northeast Central areas are the centers of Experimental Psychology—producing over 76 per cent of the work in this field. Clinical Psychology is more highly concentrated in the Middle Atlantic and Northeast Central districts which together produce over 53 per cent of the clinical studies. New England joins the last two districts as producers of Theoretical work, inasmuch as over 74 per cent of the Theoretical manuscripts come from this relatively small area. Educational Psychology is centered in the Middle Atlantic and Northeast Central areas (over 61 per cent); Abnormal Psychology in the New England and Middle Atlantic areas (over 51 per cent); Applied Psychology in the Middle Atlantic and Northeast Central areas (over 77 per cent with the Middle Atlantic area alone producing over 50 per cent). For Social Psychology the South Atlantic area first comes into the returns and with the Middle Atlantic and Northeast Central areas produces over 70 per cent. Historical Psychology is in the hands of the New England, Middle Atlantic and Northeast Central areas (over 82 per cent). Contributions to Animal Psychology are more widely spread throughout the country. The small number of titles for Statistics and Child Psychology make the results of less significance.

In summary, then, the Middle Atlantic area contributes largely to all fields. New England contributes largely to all fields except Clinical, Educational, Applied and Social Psychology. Large contributions are made by the Northeast Central area to all fields except Abnormal Psychology. The South Atlantic makes relatively large contributions to Experimental and Social Psychology.

Date of Degree

The results showing the effect of the date of the last professional degree are given in Tables VIII–X. The relation of the number of publications and date of degree are given in Table VIII. It is obvious that the more senior men who

Table VIII

Number of Publications and Date of Degree

	0	1	2	3	4	5	6	7	8	9	10 or over
Prior to 1903	28	3	9	2	4	4	1	4	2	2	22
1903–1907	15	4	8	3	2	2	1	2	1	3	14
1908–1912	12	10	7	7	8	8	2	—	1	—	24
1913–1917	21	11	11	9	3	4	7	9	5	3	31
1918–1922	9	16	7	14	11	6	4	1	8	2	31
1923–1927	5	11	16	18	5	7	13	6	3	6	10

have obtained their degrees prior to 1903 have the greatest tendency to produce nothing at all—over 34 per cent as against only 8 per cent for the 1918–1922 period. It will be remembered that practically all individuals in the 1923–1927 period and most of them in the 1918–1922 period were not academically matured for publication during the entire ten-year period under consideration. On the whole, the results show greater productivity for the individuals of more recent degree but the differences are very slight. The averages of those producing ten or more manuscripts for the six degree-granting periods are, in order: 27 per cent, 25 per cent, 30 per cent, 27 per cent, 28 per cent, and 10 per cent. This shows a surprisingly constant percentage of the maximally producing individuals for all groups except the last, which is competing on a 1–5 year basis with the others on a ten-year basis.

In Table IX are the results for consistency of publication and date of degree. Again the date of degree does not seem to have much effect upon consistency of publication. There are about as great a percentage for each degree age group who produce little or nothing and for whom publication is more or less a casual affair and also an almost equal percentage of

individuals who publish consistently over the period of years considered.

Table IX

Consistency of Publication and Date of Degree

	1	2	3	4	5	6	7	8	9	10
Prior to 1903	5	9	3	9	6	4	4	7	4	4
1903–1907	4	6	5	4	2	4	3	6	1	3
1908–1912	11	14	8	9	3	4	2	7	6	2
1913–1917	15	8	14	9	10	14	5	7	8	3
1918–1922	18	12	21	10	11	6	11	8	2	1
1923–1927	16	23	23	21	6	4	—	—	—	—

In Table X are the results showing the relation of field of publication and date of degree. Experimental Psychology is largely in the hands of those individuals who took their degrees subsequent to 1908. Clinical Psychology seems to be more evenly distributed with regard to date of degree with a centering of interest in the 1913–1922 group. For Theoretical Psychology, there is a tendency for the senior men to be responsible. Little is published by those individuals who took their degree subsequent to 1917. The group from 1913–1917 publish almost twice as many theoretical books and papers as any other group. It would seem that from 10 to 15 years from one's professional degree is the time when one's systematic and theoretical ideas are most apt to mature. Educational Psychology is largely produced by the groups 1913–1922 and with the senior group prior to 1903. Applied Psychology is very definitely in the hands of the relatively junior men. This is also true of Animal Psychology. Abnormal and Social Psychology seem to be relatively evenly distributed among the different age groups.

Place of Degree

In the former study we discovered that four institutions (Columbia, Chicago, Harvard and Clark) are responsible for 53 per cent of the American Ph.D.'s in psychology. These four institutions plus six more (Cornell, Iowa, Hopkins, Pennsylvania, Yale and Stanford) are responsible for 80 per cent. It seems worth while to analyse the publications of

Table X

Field and Date of Degree

	Experimental	Clinical	Theoretical	Educational	Abnormal	Applied	Social	Historical	Animal	Statistics	Child
Prior to 1903	73	111	101	100	68	13	45	30	15	2	2
1903–1907	84	78	97	48	27	11	17	25	4	2	6
1908–1912	170	107	99	45	62	50	39	38	11	8	8
1913–1917	156	155	200	96	28	59	46	62	39	20	20
1918–1922	162	157	48	90	46	79	46	30	54	29	9
1923–1927	146	111	42	40	32	34	25	20	33	8	15

individuals *trained* in these different institutions. The results are to be found in Tables XI–XIII.

Table XI

Place of Degree and Number of Publications

Institution	Number of Authors	Number of Publications	Average Publications
Columbia	108	708	6.56
Harvard	51	411	8.06
Clark	53	278	5.25
Chicago	67	489	7.33
Cornell	33	321	9.73
Pennsylvania	27	135	5.00
Hopkins	25	165	6.30
Yale	18	132	7.33
Stanford	21	151	7.19
Iowa	20	106	5.30
Other American	79	513	6.49
Foreign	39	331	8.49
Averages			6.91

The results for the number of publications are recorded in Table XI. The averages are in the last column and in the bottom row of this column we find a total average of 6.91 papers over the ten-year period. Thus Columbia, Clark, Pennsylvania, Hopkins, Iowa and the 'Other American' institutions fall below the average. The rank order with regard to average volume of publication is Cornell, Foreign, Harvard, Chicago, Yale, Stanford, Columbia, 'Other American,' Hopkins, Iowa, Clark and Pennsylvania.

Almost a similar distribution is found for consistency of publication in the results reported in Table XII. It would

Table XII

Place of Degree and Consistency of Publication

	1	2	3	4	5	6	7	8	9	10
Columbia	19	15	17	10	6	8	7	7	2	3
Harvard	4	12	4	6	3	4	1	4	5	1
Clark	4	5	6	4	6	6	1	5	—	—
Chicago	10	8	10	7	6	4	5	2	2	3
Cornell	1	2	4	7	4	2	3	2	4	—
Pennsylvania	6	6	8	1	—	—	1	2	—	—
Hopkins	6	3	1	3	3	3	2	1	1	—
Yale	—	2	1	1	1	2	—	1	2	1
Stanford	—	5	2	4	2	1	2	—	1	—
Iowa	3	2	5	3	1	1	—	1	—	—
Other American	13	13	11	12	5	3	1	4	3	2
Foreign	2	3	3	3	2	4	1	4	3	2

seem, from a consideration of the data of these two tables, that certain institutions are much more apt to instill a research attitude and research habits during their training than are other institutions.

In Table XIII will be found an analysis of fields of publication with regard to place of degree. For Experimental Psychology, Cornell stands supreme as a producer in this field. Except for the outstanding case of Cornell, the experimental work seems to be scattered among all of the other degree-granting institutions. Columbia and Chicago have produced many of the Clinical studies (over 37 per cent). Cornell and Columbia are the only institutions which show a great discrepancy between the number of Experimental and Clinical studies—Cornell produces almost nine times as many experimental studies as clinical and Columbia produces over twice as many clinical studies as experimental. In the field of Theoretical Psychology, Chicago, Clark, Columbia and Cornell show the greatest activity (over 59 per cent). In Educational Psychology, Columbia is outstanding with regard to mere bulk of publication—producing more than 40 per cent of the contributions in this field. Columbia, Chicago, 'Other American' and Foreign are the producers of Abnormal Psychology (67 per cent). Applied Psychology is produced by Columbia, Clark and 'Other American' (over 72 per cent). One finds that Cornell, Hopkins, Yale and Stanford are particularly weak in this field. Chicago and Columbia are especially strong in the field of Social Psychology (over 26 per cent) although the contributions here are well scattered over the group of institutions. Animal Psychology is best represented by Columbia, Clark, Chicago, Hopkins and 'Other American' (over 81 per cent). The fields of Historical and Child Psychology and Statistics are well scattered among the group of institutions.

In summary, then, so far as mere bulk of publication is concerned Columbia is in the 'honor' list for Clinical, Theoretical, Educational, Abnormal, Applied, Social, and Animal Psychology; Chicago for Clinical, Theoretical, Abnormal, Social, and Animal Psychology; Clark for Theoretical,

Table XIII

Fields and Place of Degree

	Experimental	Clinical	Theoretical	Educational	Abnormal	Applied	Social	Historical	Animal	Statistics	Child
Columbia	83	180	81	167	39	62	39	23	24	16	16
Harvard	53	57	32	28	12	19	18	9	8	2	7
Clark	67	63	96	25	17	59	28	17	27	11	10
Chicago	76	100	111	39	53	14	45	30	24	2	6
Cornell	159	18	62	15	11	—	7	29	8	5	4
Pennsylvania	31	48	12	5	8	12	4	12	1	1	2
Hopkins	30	20	37	12	18	—	14	13	26	2	1
Yale	22	33	20	24	5	3	5	15	1	2	—
Stanford	13	64	9	24	5	3	8	11	3	8	3
Iowa	5	8	5	10	10	7	7	3	2	—	3
Other American	151	91	55	32	35	51	21	25	32	16	3
Foreign	52	60	73	28	50	8	25	22	8	1	3

Applied, and Animal Psychology; Cornell for Experimental and Theoretical Psychology and Hopkins for Animal Psychology.

Size of Town

There has been a great deal of discussion recently among educators and educational administrators regarding the ideal size of town for the location of a college or university. The answer to certain aspects of this question can be found in Tables XIV–XVI. The size of towns was taken from the 1920 census. Thus the last row includes only New York City, Chicago, and Philadelphia with populations over 1,000,000.

Table XIV

Number of Publications and Size of Town

	0	1	2	3	4	5	6	7	8	9	10 or over
Under 6,000	14	12	10	7	3	1	2	1	2	—	11
6,000– 25,000	19	8	12	14	9	6	7	2	4	1	28
25,000– 100,000	10	4	7	4	3	1	2	4	2	2	11
100,000– 250,000	8	6	4	6	2	1	3	2	1	4	18
250,000–1,000,000	19	11	13	12	11	13	7	6	8	2	29
Over 1,000,000	20	13	11	8	5	9	5	8	4	7	31

Table XIV gives the number of publications in regard to size of town. On the whole, the towns with a population greater than 250,000 are responsible for the greatest productivity with the exception of the group with populations from 6,000 to 25,000. The general conclusion from a consideration of the last column (individuals producing ten or more manu-

Table XV

Consistency of Publication and Size of Town

	1	2	3	4	5	6	7	8	9	10
Under 6,000	14	11	7	4	1	5	—	5	2	—
6,000– 25,000	12	18	15	13	8	6	5	7	4	4
25,000– 100,000	6	8	7	2	4	3	5	3	2	—
100,000– 250,000	6	5	10	3	5	7	1	4	4	2
250,000–1,000,000	14	21	12	21	8	7	6	6	8	3
Over 1,000,000	15	14	14	18	10	11	7	6	2	4

scripts) is that productive research may be carried on irrespective of the size of the town in which the research worker is located.

Similar conclusions are evident for consistency of publication as will be observed from a study of Table XV. Again consistent publication may be expected from a town of any size but slightly greater consistency is evident from the larger cities.

Certain significant differences for field of research and size of town are disclosed in Table XVI. Experimental Psychology is largely the product of towns neither too large nor too small—the two groups 6,000–25,000 and 250,000–1,000,000 are responsible for over 59 per cent of experimental research. One may therefore conclude that Experimental Psychology is relatively independent of the size of the town and is largely dependent upon the attitude of the laboratories in the different institutions. Clinical Psychology is, however, largely a big city affair, inasmuch as the two groups of cities over 250,000 have produced 59 per cent of the research papers in this field. In Theoretical Psychology the results are more evenly distributed with the group 6,000–25,000 population in the clear lead. Educational Psychology is again a big city proposition, with the two groups over 250,000 responsible for 59 per cent of this work. The same is true of the big city responsibility for Abnormal Psychology with 49 per cent of the work produced in cities over 250,000. For Applied Psychology the slogan is 'the bigger the better' for in this field the more than 1,000,000 group (New York, Chicago, and Philadelphia) produced more than 47 per cent of the papers. Work in all of the other fields seems to be evenly distributed between the towns of different size.

In summary, then, any size of town is capable of contributing to any field. On the whole, it would seem that the larger towns (those over 250,000 population) are largely responsible for the development of the more applied aspects of psychology, such as Clinical, Educational, Abnormal, and Applied Psychology. If one might hazard a guess by way of interpretation one would suspect that this is largely due to

TABLE XVI

FIELD AND SIZE OF TOWN

	Experimental	Clinical	Theoretical	Educational	Abnormal	Applied	Social	Historical	Animal	Statistics	Child
Under 6,000	66	75	23	28	8	18	29	11	36	8	5
6,000– 25,000	217	85	179	67	50	26	59	65	17	3	6
25,000– 100,000	78	46	45	37	16	23	20	27	15	10	7
100,000– 250,000	95	89	97	36	9	18	30	36	29	5	3
250,000–1,000,000	254	246	100	92	112	40	32	29	23	18	21
Over 1,000,000	85	183	134	150	67	114	41	43	32	31	10

the greater opportunity for such work and to the greater wealth of clinical material in the larger cities as compared with the smaller towns.

Summary

A study of the publications of the Members of the American Psychological Association listed in the Psychological Index over the ten-year period, 1919–1928 indicates the following:

1. There is a marked sex difference with regard to average number of publications and consistency of publication in favor of the men. This sex difference is true, in varying degrees, for all fields except Child Psychology. In every other field the men contribute more than their proportionate share of publications.

2. With regard to geographical distribution, the New England, Middle Atlantic, and Northeast Central areas are relatively the great productive regions in psychology, with the South Atlantic and Pacific areas as poor seconds. Certain slight variations from the foregoing conclusion are discovered for particular fields.

3. There seems to be no great effect of the date of the last professional degree on productivity, although there seems to be more productivity and a tendency toward greater consistency of productivity of publication for those individuals who have received the degree more recently. With regard to field the date of degree seems to be effective, in favor of the relatively junior individuals, in the fields of Experimental, Applied, and Animal Psychology. The group 10 to 15 years from their degree seem to be the greatest producers of Theoretical Psychology.

4. The place of the professional degree has a marked effect upon productivity and consistency of publication. It would seem that certain institutions tend to instill the research attitude and habits of research more adequately than others. Although practically all of the major departments have contributed to all fields, there are marked individual differences in the contributions of their graduates to certain fields in some cases.

5. The size of town in which the individual is located seems to have something of an effect on productivity and consistency of publication. This is marked for the various applied fields (Clinical, Educational, Abnormal, and Applied Psychology) which are largely in the hands of workers living in cities of over 250,000 population.

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American Journal of Psychology—Ithaca, N. Y.; Cornell University.
Subscription $6.50. 624 pages annually. Edited by M. F. Washburn, K. M. Dallenbach, Madison Bentley, and E. G. Boring.
Quarterly. General and experimental psychology. Founded 1887.

Journal of Genetic Psychology—Worcester, Mass.; Clark University Press.
Subscription $7.00. 600 pp. annually. Edited by Carl Murchison.
Quarterly. Child behavior, animal behavior, comparative psychology. Founded 1891.

Psychological Review—Princeton, N. J.; Psychological Review Company.
Subscription $5.50. 540 pages annually. Edited by Howard C. Warren.
Bi-monthly. General psychology. Founded 1894.

Psychological Monographs—Princeton, N. J.; Psychological Review Company.
Subscription $6.00 per vol. 500 pages. Edited by Raymond Dodge.
Without fixed dates, each issue one or more researches. Founded 1895.

Psychological Index—Princeton, N. J.; Psychological Review Company.
Subscription $4.00. 300–400 pages. Edited by Walter S. Hunter.
An annual bibliography of psychological literature. Founded 1895.

Psychological Bulletin—Princeton, N. J.; Psychological Review Company.
Subscription $6.00. 720 pages annually. Edited by Edward S. Robinson.
Monthly (10 numbers). Psychological literature. Founded 1904.

Archives of Psychology—New York, N. Y.; Columbia University.
Subscription $6.00. 500 pages annually. Edited by R. S. Woodworth.
Without fixed dates, each number a single experimental study. Founded 1906.

Journal of Abnormal and Social Psychology—Eno Hall, Princeton, N. J.; American Psychological Association.
Subscription $5.00. 448 pages annually. Edited by Henry T. Moore.
Quarterly. Abnormal and social. Founded 1906.

Psychological Clinic—Philadelphia, Pa.; Psychological Clinic Press.
Subscription $3.00. 288 pages. Edited by Lightner Witmer.
Without fixed dates (9 numbers). Orthogenics, psychology, hygiene. Founded 1907.

Psychoanalytic Review—Washington, D. C.; 3617 10th St., N. W.
Subscription $6.00. 500 pages annually. Edited by W. A. White and S. E. Jelliffe.
Quarterly. Psychoanalysis. Founded 1913.

Journal of Experimental Psychology—Princeton, N. J.; Psychological Review Company.
Subscription $6.00. 500 pages annually. Edited by Samuel W. Fernberger.
Bi-monthly. Experimental psychology. Founded 1916.

Journal of Applied Psychology—Baltimore, Md.; Williams & Wilkins Company.
Subscription $5.00. 400 pages annually. Edited by James P. Porter.
Bi-monthly. Founded 1917.

Journal of Comparative Psychology—Baltimore, Md.; Williams & Wilkins Company.
Subscription $5.00. 500 pages ann. Ed. by Knight Dunlap and Robert M. Yerkes.
Bi-monthly. Founded 1921.

Comparative Psychology Monographs—Baltimore, Md.; The Johns Hopkins Press.
Subscription $5.00. 400 pages per volume. Knight Dunlap, Managing Editor.
Published without fixed dates, each number a single research. Founded 1922.

Genetic Psychology Monographs—Worcester, Mass.; Clark University Press.
Subscription $7.00 per volume of 500–600 pages. Two volumes per year. Edited by Carl Murchison. Monthly. Each number one complete research. Child behavior, animal behavior, and comparative psychology. Founded 1925.

Psychological Abstracts—Eno Hall, Princeton, N. J.; American Psychological Association.
Subscription $6.00. 700 pages annually. Edited by Walter S. Hunter.
Monthly. Abstracts of psychological literature. Founded 1927.

Journal of General Psychology—Worcester, Mass.; Clark University Press.
Subscription $7.00. 500–600 pages annually. Edited by Carl Murchison.
Quarterly. Experimental, theoretical, clinical, historical psychology. Founded 1927.

Journal of Social Psychology—Worcester, Mass.; Clark University Press.
Subscription $7.00. 500–600 pages ann. Ed. by John Dewey and Carl Murchison.
Quarterly. Political, racial, and differential psychology. Founded 1929.